PHILOSOPHICAL

TRANSACTIONS

OF THE

ROYAL SOCIETY

OF

LONDON.

FOR THE YEAR MDCCCXLVII.

PART I.

LONDON:

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MDCCCXLVII.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the Council-books and Journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries till the Forty-seventh Volume; the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued, for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly enlarged, and their communications more numerous, it was thought advisable that a Committee of their members should be appointed, to reconsider the papers read before them, and select out of them such as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgement of their respective authors.

It is likewise necessary on this occasion to remark, that it is an established rule of the Society, to which they will always adhere, never to give their opinion, as a Body,

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upon any subject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently proposed from the Chair, to be given to the authors of such papers as are read at their accustomed meetings, or to the persons through whose hands they received them, are to be considered in no other light than as a matter of civility, in return for the respect shown to the Society by those communications. The like also is to be said with regard to the several projects, inventions, and curiosities of various kinds, which are often exhibited to the Society; the authors whereof, or those who exhibit them, frequently take the liberty to report and even to certify in the public newspapers, that they have met with the highest applause and approbation. And therefore it is hoped that no regard will hereafter be paid to such reports and public notices; which in some instances have been too lightly credited, to the dishonour of the Society.

The Meteorological Journal hitherto kept by the Assistant Secretary at the Apartments of the Royal Society, by order of the President and Council, and published in the Philosophical Transactions, has been discontinued. The Government, on the recommendation of the President and Council, has established at the Royal Observatory at Greenwich, under the superintendence of the Astronomer Royal, a Magnetical and Meteorological Observatory, where observations are made on an extended scale, which are regularly published. These, which correspond with the grand scheme of observations now carrying out in different parts of the globe, supersede the necessity of a continuance of the observations made at the Apartments of the Royal Society, which could not be rendered so perfect as was desirable, on account of the imperfections of the locality and the multiplied duties of the observer.

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That the Royal Medals be given for such papers only as have been presented to the Royal Society, and inserted in their Transactions.

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1. Astronomy; Physiology, including the Natural History of Organized Beings.
2. Physics; Geology or Mineralogy.
3. Mathematics; Chemistry.

That, in case no paper, coming within these stipulations, should be considered deserving of the Royal Medal, in any given year, the Council have the power of awarding such Medal to the author of any other paper on either of the several subjects forming the Cycle, that may have been presented to the Society and inserted in their Transactions; preference being given to the subjects of the year immediately preceding: the award being, in such case, subject to the approbation of Her Majesty.

The Council propose to give one of the Royal Medals in the year 1847 for the most important unpublished paper in Mathematics, communicated to the Royal Society for insertion in their Transactions after the termination of the Session in June 1844, and prior to the termination of the Session in June 1847.

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The Council propose also to give one of the Royal Medals in the year 1850 for the most important paper in Chemistry, communicated to the Royal Society after the termination of the Session in June 1846, and prior to the termination of the Session in June 1849, and printed in the Philosophical Transactions.

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PHILOSOPHICAL TRANSACTIONS.

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I. The Bakerian Lecture.—*On certain Phenomena of Voltaic Ignition and the Decomposition of Water into its constituent Gases by Heat.*

By W. R. Grove, *Esq., M.A., F.R.S.*

Received September 3,—Read November 19, 1846.

In the Philosophical Magazine for August 1841, I recommended for eudiometrical purposes, the use of a platinum wire ignited by a voltaic battery. In Plate I. fig. 1, is represented a form of apparatus for this purpose; it consists of a tube of Bohemian glass, with a loop of platinum wire $\frac{1}{80}$th of an inch diameter sealed into its upper end; the size of the glass tube may be adapted to the quantity of gas sought to be analysed, and may when necessary be reduced to extremely small dimensions, one-eighth of an inch being ample; into this the gas may readily be made to ascend, by the insertion of a wire of copper, platinum, or glass, as may be suitable to the gas: two cells of the nitric-acid battery are sufficient fully to ignite the wire, and the same battery supplies, by electrolysis, pure oxygen and hydrogen for the analysis. Since the period when I first proposed this, I have seldom used any other apparatus for such gaseous analyses as are performed by combining the gas to be examined with oxygen or hydrogen. This eudiometer possesses the advantage of enabling the operator either to detonate or slowly to combine the gases, by using different powers of battery, by interposing resisting wires, or by manipulation alone,—a practised hand being able by changing the intervals of contact to combine or detonate the gas at will. My general practice has been to produce a gentle heat in the wire until the gases contract, and then gradually to increase the heat until a full ignition takes place, by which means all the objects of the eudiometer of Volta are fulfilled, without detonation, without dependence on the fickle electric spark, and without thick tubes, any danger of explosion, or of the gases being projected from the eudiometer.

I have commenced with a description of this eudiometer, as it has been indirectly the means of my undertaking the experiments detailed in this lecture; and as its very great convenience has never been generally understood, I think that in strongly recommending it, I shall be of service to chemists.

In a paper honoured by insertion in the Philosophical Transactions for 1845, p. 358, I have shown another method of eudiometry also performed by voltaic ignition; in that experiment the vapour of camphor was decomposed into carbonic oxide and carburetted hydrogen; it was an application of voltaic ignition to effects analogous to those produced by Priestley and others, by passing compound gases through ignited tubes of porcelain.

But the voltaic process has this immense advantage, that the heat can be rendered incomparably more intense; that the quantity of vapour or gas to be operated on may be indefinitely small; that there are no joints, stop-cocks or ligatures; and that there is no chance of endosmose, which takes place through all porcelain vessels. I therefore determined to examine by these means several gases, both with a view of verifying, under different circumstances, known results, and seeking for new effects by this new and advantageous application. I used an eudiometer (fig. 1) of 8 inches long and 0·4 inch internal diameter, exposing the gases to intense heat, and subsequently analysed the residues in one of the same length, but 0·2 inch diameter.

I will first consider the physical effects of different gases on the ignition of the wire itself.

In a paper on the Application of Voltaic Ignition to lighting Mines*, I have mentioned the striking effects of hydrogen in reducing the intensity of ignition of a platinum wire, so much so that a wire voltaically ignited to incandescence in atmospheric air, is apparently extinguished by inverting over it a jar of hydrogen; with other gases the effects are not so striking, and with them these differences are best shown by including a voltameter in the circuit. Davy found that the conducting power of a wire diminished in proportion to the degree to which it was heated: assuming the accuracy of this position, the amount of gas in the voltameter would be inverse to the intensity of ignition in the wire. The following is the result I obtained with different gases, employing the same battery (the nitric-acid combination at its most constant period), the same wire, and the same vessel:—

Gases surrounding the wire.	Cubic inches of gas evolved in the voltameter, per minute.
Hydrogen	7·7
Olefiant gas	7·0
Carbonic oxide	6·6
Carbonic acid	6·6
Oxygen	6·5
Compressed air, 2 atmospheres	6·5
Nitrogen	6·4
Atmospheric air	6·4
Rarefied air	6·3
Chlorine	6·1

* Philosophical Magazine, December 1845.

To ascertain the relation between the amount of radiant heat generated by the same battery and wire in gases which presented striking differences as to the luminous effects of the platinum wire, an apparatus was prepared in which the bulb of a thermometer was retained at a certain distance from the coil of wire ignited by a battery of four cells, and exposed, first, to an atmosphere of hydrogen, and then to one of atmospheric air, at the same temperature and pressure; the thermometer rose $7\frac{1}{2}°$ in five minutes in the hydrogen, and 15° in the air in the same time. Both the heating and luminous effects appear therefore to be greater in atmospheric air than in hydrogen. I cannot satisfactorily account for the differences shown in the above table; there appears a general tendency to greater ignition in the electronegative than in the combustible gases, but the facts are far too few to found a generalization. I was at first inclined to regard the difference of effect in hydrogen as analogous to the peculiarity mentioned by Leslie* respecting its convection of sound, but the parallel does not hold; sound is transmitted imperfectly through rarefied air, and also through hydrogen; on the contrary, the heat of the ignited wire is most intense in the former, and least so in the latter; the heat is also very much reduced in intensity in the compounds of hydrogen, ammonia and olefiant gas, or even by a small admixture of hydrogen with another gas, such as nitrogen; hydrogen, therefore, appears to have a peculiar and specific action in this respect.

I now pass to the consideration of the effects of the ignited wire on different gases. The ignition was in every case raised to the fullest extent, and the gases after exposure to it were carefully cooled down to their original temperature.

When the experiments were made over water, the whole eudiometer was immersed in a vessel of distilled water, occasionally having an inch depth of oil on the surface (see fig. 2†); when over mercury, and a long-continued exposure was required, a bent tube was employed, as at fig. 3, the closed end being immersed in water or oil, to prevent the fusion of the glass which would otherwise have ensued.

The tubes are much more easily preserved from cracking, and the ignition better kept up with oil on the exterior than with water, but as in many of these experiments I might have been considerably misled by a crack in the glass, or a bad sealing of the wire, allowing a portion of oil to enter the tube, I used water in the greater number of them until I was assured of the phenomena.

The apparatus, fig. 3, is superior in one respect to fig. 2, even for experiments over water, as the wire being situate below the volume of gas, the circulation is more rapid. This object may also be effected by employing the form of eudiometer, fig. 4, in which the loop of wire is near the centre of the tube, so as to be just above the surface of water in the tube; there are, however, some difficulties of manipulation with this form, which render it practically of less value than fig. 1.

* Transactions of the Cambridge Philosophical Society, vol. i. p. 267.

† In this and in figs. 3 and 5, the lines leading from the platinum loop to the mercury cups represent copper wires.

Binoxide of nitrogen over distilled water contracted differently in proportion to the heat of the wire; in the best experiment it contracted to one-third of its original volume; the residual gas was nitrogen. Nitric acid was found in solution in the water.

Over mercury the effects were nearly the same; the mercury was attacked, and the orange fumes of nitrous acid were visible.

Protoxide of nitrogen was decomposed into nitrogen and oxygen; the volume increased by 0·35 of the original volume; I could not get the full equivalent proportion, or 0·5 of oxygen.

Carbonic acid underwent no perceptible alteration.

Ammonia increased to double its original volume; it was now no longer absorbable by water, and gave 3 volumes of hydrogen, plus 1 nitrogen.

Olefiant gas contracted slightly, deposited carbon, the residue being hydrogen and olefiant gas, more of the former in proportion to the heat, but I could not succeed in entirely decomposing it.

Nitrogen suffered no change.

Oxygen gave a very slight contraction, amounting to $\frac{1}{50}$th of its volume; the oxygen employed was very pure, obtained from chlorate of potash and manganese, and also from water by electrolysis: no change in properties was perceptible in the oxygen after its exposure to the ignited wire. This contraction I incline to attribute to a slight portion of hydrogen present, which view will, I think, be considered as strengthened by the effect of the ignited wire on hydrogen, to be presently detailed. I at one time thought that the contraction might be due to a slight oxidation of the wire, but it never went beyond a very limited point; nor was the wire altered in size or weight, though it was kept ignited for many hours.

Chlorine over water gave dense white fumes; a greyish yellow powder accumulated on the sides of the tube near the platinum wire, which appeared of the same nature as the vapours; the deposit was insoluble in cold nitric, sulphuric, or muriatic acid, but dissolved by the last when boiled. The fumes did not, as far as I could judge, affect litmus paper; a barely perceptible tinge of red was indeed communicated to it, but this, I had every reason to believe, was attributable to a slight portion of muriatic acid not absorbed by the water. I have not yet worked out this result, as it is probable, considering the number of experiments that have been made on heated chlorine, that it is a known product, though I cannot find, in several books to which I have referred, any substance answering to it in description, and the field opened by voltaic ignition is so new that each result demands a separate and prolonged examination; if I find that this is an unknown compound I shall probably resume its investigation*.

Cyanogen gave, though in very minute quantities, a somewhat similar deposit, but at its then very high temperature it began to act rapidly on the mercury, and I was obliged to give up the experiment after an hour's ignition. Both these gases require peculiar

* See Supplemental Paper, p. 20.

and novel apparatus for examination by voltaic ignition. It will presently be seen that my whole attention and disposable time were necessarily occupied with certain phenomena to which this class of experiments ultimately led me.

Hydrogen gave a very notable contraction, amounting in some cases to one-tenth of its volume. This was an unexpected result, and I examined it with care. It took place both over water and over mercury; rather more with the former than with the latter. It obtained equally with hydrogen procured by electrolysis from carefully distilled water and pure sulphuric acid; with that procured from common zinc and pure sulphuric acid diluted with distilled water; and with that obtained from distilled zinc and pure diluted sulphuric acid. The contraction was less when the water from which the hydrogen was obtained was carefully purged of air by boiling and the air-pump, but yet there was a notable contraction even when the water had been freed from air to the utmost practicable extent. In the numerous experiments which I made on this subject, the contraction varied from the $\frac{1}{10}$th to the $\frac{1}{30}$th of the whole volume.

After many fruitless experiments I traced it to a small quantity of oxygen which I found hydrogen to contain under all circumstances in which I examined it. Phosphorus placed in hydrogen, obtained with the utmost care, gives fumes of phosphorous acid, shines in the dark and produces a slight contraction, but there is after this a further contraction by the use of the ignited wire.

I may cite the following as an easy experiment and simple illustration of the rapidity with which hydrogen appropriates oxygen. Let hydrogen be collected over water well-purged of air; let a piece of phosphorus remain in it until all combustion has ceased, the hydrogen will then be full of phosphoric vapour; fill another tube with water, and pass the hydrogen rapidly into it, the second tube will instantly be filled with a dense white fume of phosphorous acid; the hydrogen having instantly carried with it oxygen from the stratum of water it has passed.

A very careful experiment was made as follows:—distilled water was boiled for several hours, to this was added one-fortieth part by measure of pure sulphuric acid, and it was cooled under the receiver of an air-pump; it was now placed in two test glasses, connected by a narrow inverted tube, full of the same liquid: platinum electrodes were placed in each glass, and the hydrogen caused to ascend immediately into the eudiometer tubes; the whole was completed within two or three minutes after the water had been removed from the air-pump. Here the ordinary sources of impurity in hydrogen were avoided; no zinc was used, the sulphuric acid was pure, and the quantity was so small, that, had it not been pure, the error could have been but very trifling. The hydrogen so obtained, contracted in volume $\frac{1}{30}$th; hydrogen prepared in the same way, and exposed to phosphorus, gave dense white fumes; the phosphorus was luminous in the dark for more than an hour, and the contraction (temperature and pressure being carefully examined) was $\frac{1}{90}$th; the amount of contraction by the wire would of course equal three times the volume of oxygen mixed with the hydrogen, consequently the oxygen would be $\frac{1}{7}$th of the whole volume;

the platinum wire induces therefore a greater absorption of oxygen than the phosphorus, unless the volume of hydrogen is increased by the phosphoric vapour; the sequel of this paper will render it probable that even the ignited wire does not, and cannot induce combination of all the oxygen existing in the hydrogen.

I have looked into the papers of MM. Berzelius and Dulong, and of M. Dumas on the equivalent weight of hydrogen. The latter contains a most careful experimental investigation, and is by far the best determination we have; although it is not there mentioned that hydrogen contains oxygen, yet a correction is made for the air contained in the sulphuric acid employed. M. Dumas does not state how the quantity of that air is calculated. There can be no question that nothing approaching in elaborate care to these experiments has been yet performed on the subject, but with the fullest consciousness of M. Dumas' skill, I have, in all my experiments, perceived such an inveterate tendency of hydrogen to possess itself of oxygen, that I cannot help entertaining some doubts whether we have yet the real weight of hydrogen within the assigned limits of error.

It is difficult to see how hydrogen can be absolutely deprived of oxygen which has once existed in it; neither an oxidable metal as zinc, or an ignited inoxidable metal as platinum, getting rid of all the oxygen, and phosphorus, if it does so, replaces it by its own vapour. The near approach, however, of the equivalent of hydrogen, as determined by M. Dumas, to the ratio of whole numbers, renders it probable that it is a very close approximation to the truth.

I have not been able to detect nitrogen in the hydrogen, but the probability is that a slight quantity also exists in it. Whether the oxygen proceeds from portions of air still remaining in solution in the liquid from which the air is exhausted, or whether it is a part of the water actually decomposed, but of which the oxygen is not absorbed by the zinc, is a question to resolve which further experiments are necessary.

Hydrogen and carbonic acid mixed in equal volumes were readily acted on by the ignited wire; they contracted to 0·48 of the original volume; the residue was carbonic oxide; one equivalent of oxygen had therefore united with the hydrogen; and the slight additional contraction was probably due to the further combination of hydrogen with oxygen, as above stated.

Carbonic oxide exhibited a remarkable effect, and one which, coupled with the last experiment, gave rise to considerations which mainly led to the results to be detailed in the body of this paper. Carbonic oxide, very pure and carefully freed from carbonic acid, was exposed to the ignited wire over distilled water; the gas increased in volume in one experiment to one-third of its original volume, in the greater number of instances to one-fifth: this increase depended upon the intensity of ignition, which it was very difficult to maintain at its maximum on account of the frequent fusions of the platinum wires.

Here again I had a long research and many erroneous guesses, which I need not

detail. The effect did not take place with perfectly dry gas over mercury, and I thence was led to attribute it to some combination with aqueous vapour; the increase turned out to be occasioned by the formation of carbonic acid. By agitation with caustic potash or lime water the gas was reduced to exactly its original bulk, but it was now found to be mixed with a volume of hydrogen equal to the volume of carbonic acid by which it had been increased; it was thus perfectly clear that half a volume or one equivalent of oxygen derived from the vapour of the water, had combined with one volume or equivalent of carbonic oxide, and formed one volume or equivalent of carbonic acid, leaving in place of the carbonic oxide with which it had combined, the one volume or equivalent of hydrogen with which it had been originally associated.

Comparing the last experiment, viz. that of mixed carbonic acid and hydrogen with this, I was naturally struck with the curious reversal of affinities under circumstances so nearly similar; in the one case, hydrogen taking oxygen from carbonic acid to form water and leaving carbonic oxide; in the other, carbonic oxide taking oxygen from water to form carbonic acid and leaving hydrogen.

I thought much upon this experiment; it appeared to me ultimately that the ignited platinum had no specific effect in producing either composition or decomposition of water, but that it simply rendered the chemical equilibrium unstable, and that the gases then restored themselves to a stable equilibrium according to the circumstances in which they were placed with regard to surrounding affinities; that if the state of mixed oxygen and hydrogen gas were, at a certain temperature, more stable than that of water, ignited platinum would decompose water as it does ammonia.

This is a very crude expression of my ideas, but we have no language for such anticipatory notions, and I must adapt existing terms as well as I am able.

It now appeared to me that it was possible to effect the decomposition of water by ignited platinum; that, supposing the atmosphere of steam in the immediate vicinity of ignited platinum were decomposed, or the affinities of its constituents loosened, if there were any means of suddenly removing this atmosphere I might get the mixed gases; or secondly, if, as appeared by the last two experiments, quantity had any influence, that it might be possible so to divide the mixed gases by a quantity of a neutral ingredient as to obtain them by subsequent separation (or as it were filtration) from the neutral substance. Both these ideas were realized.

To effect the first object, after, as usual in such circumstances, much groping in the dark, I cemented a loop of platinum wire in the end of a tube retort similar to fig. 3, and covered it with asbestos, ramming this down so as to form a plug at the closed extremity of the tube, the platinum wire being in the centre. My object was, by igniting the platinum wire, to drain the water out of the asbestos, and the ignited wire being then in an atmosphere of steam, I hoped the water would by capillary attraction keep constantly oozing down to the platinum wire as the steam or decomposed water ascended. The experiment did not succeed; the water established a

current through the asbestos by washing away fine particles, and the phenomena of ordinary ebullition took place, unless the intensity of the battery was very much exalted, when a very slight decomposition was perceptible, which I attributed to electrolysis. This experiment, however, suggested another which did succeed. In one or two cases the asbestos plug became compressed above the platinum and so choked up the tube that the wire suddenly fused. It now occurred to me that by narrowing the glass tube above the platinum wire I had the result at my command, as the narrow neck might be made of any diameter and length, so as just to allow the water to drip or run down as the steam forced its way up; a tube was so formed, and is shown with its accompaniments at fig. 5.

The result of this experiment was very striking: when two cells of the nitric-acid battery were applied the air was first expanded and expelled, the water then soon boiled, and at a certain period the wire became ignited in the steam. At this instant a tremulous motion was perceptible, and separate bubbles of permanent gas of the size of pin-heads ascended, and formed a volume in the bend of the tube. It was not a continuous discharge of gas as in electrolysis, but appeared to be a series of rapid jerks; the water, returning through the narrow neck, formed a natural valve which cut off by an intermitting action portions of the atmosphere surrounding the wire; the experiment presented a novel and indescribably curious effect. The gas was oxyhydrogen. It will occur at the first to many of those who hear this paper read, that this effect might be derived from electrolysis. No one seeing it would think so for a moment; and although I shall by my subsequent experiments, I trust, abundantly negative this supposition, yet as this was my first successful experiment on this subject, and is *per se* an interesting and striking method of showing the phenomenon of decomposition by heat, I will mention a few points to prove that the phenomenon could not be occasioned by electrolysis.

To account for it by electrolysis, it must be supposed that the wire offered such a resistance to the current that this divided itself, and the excess of voltaic power passed by the small portion of water which trickled down, instead of by the wire.

In the first place, the experiment was performed with distilled water, and only two cells of the battery employed, which will not perceptibly decompose distilled water.

2ndly. No decomposition took place until the instant of ignition of the wire, though there was a greater surface of boiling water exposed to the wire before than after the period of ignition.

3rdly. A similar experiment was made, but with the wire divided in the centre so as to form two electrodes, and the water boiled by a spirit-lamp; here the current had no wire to conduct any part of it away, but the whole was obliged to pass across the liquid, and yet no decomposition took place, or if there were any it was microscopic.

4thly. When, instead of oil, distilled water was used in the outer vessel*, even the

* January 8.—I have since found that the exterior tube of oil or water may be dispensed with in this experiment, as the water which trickles down prevents the fusion of the glass.

copper wires, one of which would form an oxidable anode, gave no decomposition across the boiling water outside, while the ignited wire inside was freely yielding mixed gases.

5thly. To prevent the water from being the shortest line for the current, I repeated the experiment with a perfectly straight wire (fig. 6). The result was precisely the same; but the experiment is more difficult, as a certain length of wire is necessary, the sealing is more troublesome, and the size of the bulb is much more difficult to adapt to the production of steam in exactly the requisite quantity; the straight wire being more suddenly extinguished and more easily fused: with careful manipulation however it succeeds equally well with the former experiment.

I might add other experiments and arguments, but I believe when the remainder of this paper has been read, that the above will be thought scarcely necessary.

I now directed all my efforts to produce the effects by heat alone without the battery. I will mention a few of my unsuccessful attempts, as it will save trouble to future experimenters. I sealed a platinum wire into the extremity of a curved tube, filled the latter with water, and applied a strong heat by the blowpipe to the projecting end of the wire, hoping that the conducting power of the platinum, although inferior to that of most other metals, was sufficiently superior to that of glass to enable me to ignite the portion of the wire within the tube, and thus surround it with an atmosphere of steam; the water however all boiled off from the glass; nor could I succeed in igniting the platinum by heat from without. A similar failure occurred when, on account of its superior conducting power, a gold wire was substituted for that of platinum.

I sealed spongy platinum and bundles of platinum wire into the ends of Bohemian glass tubes, closing the glass over them, and then filling the tubes with water and heating the whole extremity; but the water boiled off from the glass, and the platinum could not be made to attain a full incandescence.

After many similar trials I returned to the battery, and sought to apply it in a manner in which electrolysis could not possibly take place. I had hoped, as I have above stated, to obtain a residual decomposition of water by masking or diluting the gases by a neutral substance. I therefore tried the following experiment: a tube similar to fig. 1 was filled with water which had been carefully freed from air by long boiling and the air-pump; it was then inverted in a vessel of the same water, and a spirit-lamp applied to its closed extremity, until the upper half was filled with vapour (see fig. 7). The wire was brought to a full ignition by the battery, and kept ignited for a few seconds; connexion was then broken and the lamp removed, so that the water gradually ascended. A bubble of the size of a large mustard-seed was left in the extremity of the tube, and I was much gratified at finding that when this was caught by a lighted match at the surface of the water-trough it detonated. The experiment was then repeated, continuing the ignition for a longer time, but the gas could not be increased beyond a very limited quantity; indeed it was not to have been expected, as supposing it to be mixed gas, recombination of the excess would have taken place,

and the fact of any uncombined gas existing when exposed to incandescent platinum, will doubtless surprise those who hear it for the first time.

The experiment was repeated as at first and the bubble transferred to another tube; the wire was then again ignited in vapour, another bubble was instantly formed and transferred, and so on, until after about ten hours' work sufficient gas was collected for analysis; this gas was now placed in an eudiometer, it detonated and contracted to 0·35 of its original volume; the residue being nitrogen. The experiment was repeated several times with the same general results, the residue sometimes containing a trace of oxygen.

Here electrolysis was out of the question; the wire was ignited in (if I may use the expression) dry steam, the upper part of the tube being far above the boiling-point, and of course perfectly transparent; if not an effect of heat, it must have been a new function of the electric current, at least one hitherto unknown.

As the voltaic arc and electric spark afford heat of the greatest intensity, I tried a succession of electric sparks from platinum wires through steam in the apparatus fig. 8, the water, as in all my experiments, having been previously purged of air (to save circumlocution I will in future call it prepared water). The sparks were taken from the hydro-electric machine of the London Institution; they had in the steam a beautiful crimson appearance; on cooling the tube a bubble was perceptible, which detonated by the match.

As in the previous experiments, a whole day's work did not increase the bubble, but when it was transferred another instantly formed; the gas was similarly collected; it detonated and contracted to 0·4 of its original volume; the residue was nitrogen with a trace of oxygen.

This experiment will again surprise by its novelty; the very means used in every laboratory to combine the mixed gases and form water, here decompose water*. From a vast number of experiments which I have made on the voltaic and electric disruptive discharges (which are I believe similar phenomena, differing only in quantity and intensity), I believe the decompositions produced by them are the effects of heat alone, and this experiment was therefore to my mind a repetition of the last under different circumstances; others however may think differently. This experiment also I several times repeated.

By counting the globules given off, and comparing a certain number of them with the average volume of steam in the last two experiments, an attempt was made to ascertain what proportion of water could be decomposed by an ignited platinum wire in aqueous vapour, or, which amounts to a corollary from this proposition, what degree of dilution would enable mixed gas to exist without combustion in an atmosphere of steam exposed to an ignited platinum wire. The proportion in an experiment in which the globules were so counted, was 1 to 2400; the probability is however that

* I need scarcely point out the distinction, in fact, between this experiment and those in which liquid water has been decomposed by the electric spark. See Supplemental Paper, p. 21.

different temperatures of the platinum wire would give different volumes of gas so decomposed, the volume being greater as the wire is more intensely ignited.

Although there was no known effect of electricity which could produce the phenomenon exhibited by the last two experiments, and it was in any event new, still, firmly convinced that it was an effect of heat, I again determined to attempt its production by heat alone, and without the use of the battery. I procured a tube of silver 9 inches long and 0·4 inch diameter; at the extremity of this was a platinum cap to which a smaller tube, also of platinum, was soldered. This platinum tube was closed at the end and soldered with gold solder. The apparatus was filled with prepared water; the water was boiled in the tube to expel the air from the narrow tube and any which might have adhered to the vessel; the tube was then, when full of hot water, inverted into water, and the flame of a common blowpipe made to play against the platinum tube (see fig. 9) until a white heat was obtained. Upon inverting it under water, a bubble of the size of a mustard-seed rose to the surface, which gave a very feeble detonation with the match. Similar bubbles were collected as before, and the gas in an eudiometer contracted to 0·7. On repetition the experiment did not succeed so well, and upon several repetitions it sometimes succeeded and sometimes failed, and I should not mention it but that it was the first experiment which gave me, although not very satisfactorily, the effect of decomposition by heat alone. The reason of its uncertainty I believe to have been the want of a sufficiently intense heat, as I dared not venture on account of the gold solder to push the ignition very far; in fact, I subsequently fused the extremity and spoiled the apparatus by applying the oxyhydrogen flame to it; had the platinum tube been welded instead of gold-soldered, it would doubtless have succeeded better. I should state that the object of the silver tube was to prevent the chance of recomposition by the catalytic effect of a large platinum surface; to have, in short, a small portion of platinum exposed to the steam, and that at a high temperature: economy was also no indifferent consideration. This experiment, although, coupled with the previous ones, tolerably conclusive, did not satisfy me, and I attacked the difficulty in another manner. The experiment (fig. 5) induced me to believe that if I could get platinum ignited under water so as to be in an atmosphere of steam, decomposition would take place; and M. Boutigny's experiments on the spheroidal state of water led me to hope I might keep platinum for some time under conditions suitable for my purpose.

After a few failures I succeeded perfectly by the following experiment. The extremity of a stout platinum wire was fused into a globule of the size of a peppercorn, by a nitric-acid battery of 30 cells; prepared water was kept simmering by a spirit-lamp, with a tube filled with water inverted in it; charcoal being the negative terminal, the voltaic arc was taken between that and the platinum globule until the latter was at the point of fusion; the circuit was now broken, and the highly incandescent platinum plunged into the prepared water: separate pearly bubbles of gas rose into the tube, presenting a somewhat similar effect to experiment (fig. 5).

The process was repeated, the globule being frequently plunged into the water in a state of actual fusion; and when a sufficient quantity of gas was collected it was examined, it detonated, leaving 0·4 residue; this was as usual nitrogen with a trace of oxygen. A second experiment gave a still better result, the gas contracting to 0·25 of its original volume.

On making the platinum negative and the charcoal positive, a very different result followed; the carbon was, as is known to electricians, projected upon the platinum; and the gas in this case was mixed with carburetted hydrogen and carbonic oxide. I know no experiment which shows so strikingly the different effects at the disruptive terminals as this; when the platinum is negative it gives much carbonic gas, when it is positive, not a trace (the gas was delicately and carefully tested for it); nay, more, by changing the platinum from negative to positive the carbon is instantly removed, and in a single experiment the platinum becomes perfectly clean.

Here then I produced very satisfactorily decomposition by heat; it is true, the battery was used, but used only as a means of fusing the platinum, as this was, as soon as fused, entirely separated from the circuit and could have no possible voltaic action. Wishing however altogether to avoid the use of the battery, I repeated this experiment, employing as my means of fusing the platinum the oxyhydrogen blowpipe; the experiment was equally successful, perhaps more so, as the manipulation was more easy.

I could readily by this means collect half a cubic inch or more of the gas; when detonated, the residue of nitrogen averaged 0·35 of the original volume.

In carefully watching this experiment, I observed that at first a rapid succession of bubbles ascended into the tube from the incandescent platinum, it then became quiescent; the spheroidal state was assumed by the water and no gas ascended; on losing the spheroidal state a sudden hiss was heard, and a single bubble ascended into the tube. I determined to examine separately the gas from the platinum before and after the quiescent state; to effect this I placed two inverted tubes in the capsule with the orifices near each other; the platinum at the point of fusion was immersed under one tube, say tube A, and as soon as the ascent of bubbles ceased, it was removed across to tube B, and the last bubble then entered that tube; the gases from each tube were separately analysed, and tube A gave nearly all detonating gas, the residue being only 0·2; tube B gave none; the gas collected in it was nitrogen, with a trace of oxygen.

In order to examine the effect of an oxidable metal under similar circumstances, I fused by the oxyhydrogen blowpipe the end of a stout iron wire, plunged it into prepared water and collected the globules of gas; no oxygen was given off, or at least no more than I have always found to accompany hydrogen, which with a small residue of nitrogen was the gas given off in this experiment.

I was now anxious to produce a continuous development of mixed gas from water subjected to heat alone, in other words, to succeed in an experiment which should

bear the same relation to experiment fig. 9 as fig. 5 did to fig. 7; for this purpose the apparatus shown at fig. 10 was constructed: *a* and *b* are two silver tubes 4 inches long by 0·3 inch diameter; they are joined by two platinum caps to a platinum tube *c*, formed of a wire one-eighth of an inch diameter drilled through its entire length, with a drill of the size of a large pin; *a* is closed at the extremity, and to the extremity of *b* is fitted, by means of a coiled strip of bladder, the bent glass tube *d*. The whole is filled with prepared water, and having expelled the air from *a* by heat, the extremity of the glass tube is placed in a capsule of simmering water. Heat is now applied by a spirit-lamp, first to *b* and then to *a*, until the whole boils; as soon as ebullition takes place, the flame of an oxyhydrogen blowpipe is made to play upon the middle part of the platinum tube *c*, and when this has reached a high point of ignition, which should be as nearly the fusing-point of platinum as is practicable, gas is given off, which, mixed with steam, very soon fills the whole apparatus and bubbles up from the open extremity, either into the open air or into a gas collector. Although by the time I had devised this apparatus I was from my previous experiments tolerably well assured of its success, yet I experienced a feeling of great gratification when on applying a match to one of the bubbles which were ascending, it gave a sharp detonation; I collected and analysed some of it; it was 0·7 oxyhydrogen gas, the residue nitrogen, with a trace of oxygen.

Those who have endeavoured to deprive water of air, will have no difficulty in accounting for the residual nitrogen, or nitrogen mixed with a small portion of oxygen, which has occurred in all my experiments. De Luc pointed out the impossibility of practically depriving water of air, and Priestley, from observing the obstinacy with which water retained air, was led to believe that water was convertible into nitrogen (phlogisticated air). I have repeated several of Priestley's experiments under much more stringent circumstances, and have never been able to free water from air, or so to boil water that for every ebullition of vapour a minute bubble of permanent gas was not left, which appeared to have been an indispensable nucleus to the vapour.

The difficulty of boiling water increases, as M. Donny has proved, in proportion to its freedom from air, and at last the bursts of vapour become so enormous that the vessels employed are generally broken. There appears to me a point beyond which this resistance does not extend; but even at this point a minute bubble of air is left for each burst of vapour, though they are so few and distant that the aggregate amount of gas is very trifling. I have produced from water which had been previously carefully deprived of air by the ordinary methods, three-fourths of its own volume of permanent gas, which proved to be nitrogen; but as the water in this experiment was boiled under a long column of oil, it is probable that if any oxygen were present, it might have been absorbed by the oil; I have, however, always found the proportion of oxygen to decrease as the boiling was continued. It may be worth noticing, as having had some influence on my mind, that many months ago, when considering the experiments of Henry and Donny on the cohesion of water, I men-

tioned to Mr. Gassiot, and also to Mr. Bingham my assistant (to whose assiduity I am much indebted), that I was inclined to think if water could be absolutely deprived of air, it would be decomposed by heat, a result which I have now attained by a totally different series of inductions. It is a circumstance worthy of remark, that I find the greater part of the air to be expelled at a comparatively low temperature, and when the water has come in contact with the platinum, while the decomposition all takes place when the platinum is surrounded by an atmosphere of steam, if steam it may be called, for the state of this atmosphere at the first immersion of the platinum is at present very mysterious.

I think I may now safely regard it as proved, that platinum intensely ignited will decompose water, and several considerations press on the mind in reflecting on this novel phenomenon.

First of all, to those who are attached to the *cui bono* argument, and estimate physical science in proportion only to its practical applications, I would say that these experiments afford some promise of our being, at no distant period, able to produce mixed gases for purposes of illumination, &c. by simply boiling water and passing it through highly ignited platinum tubes, or by other methods which may be devised; we in fact by this means, as it were, boil water into gas, and there appears theoretically no more simple way of producing chemical decomposition.

To pass however to more important considerations: the spheroidal state, which has lately attracted the attention of philosophers, appears to be closely connected with these results, and is rendered more deeply interesting. The last experiment but two which I have mentioned, shows that the spheroidal state is intermediate between ordinary ebullition and the decomposing ebullition; it is probably therefore a state of polar tension, coordinate in some respects with that which takes place in the cell of a voltaic combination before decomposition, or when the power employed not being of sufficient intensity to produce actual decomposition, the state commonly called polarization of the electrodes, obtains. The phenomenon brings out also a new relation between heat, electricity, and chemical affinity; hitherto many electrical phenomena could be produced by heat and chemical action, the difference being that in the effects produced by the last two forces there was no polar chain, but every minute portion of the matter acted on gave rise to the phenomena which in the electrical effects are only observable at the polar extremities; thus in decomposing water by iron and sulphuric acid, or by passing steam over heated tubes of iron, parallel results are obtained to the electrolysis of water with an iron anode; but in the former cases every portion of the iron oxidated gives off its equivalent of hydrogen, in the latter the equivalent is evolved from the cathode at a point distant from that where the oxidation takes place. Hitherto electricity has been the only force by which many compounds, and particularly water, could be resolved into their constituents without either of these being absorbed by another affinity. The decomposition by ignited platinum removes this exception, and presents the parallel effect produced by heat alone.

Although there is no substance except platinum and some of the more rare metals, such as iridium, which promise much success in a laboratory experiment made for the purpose of producing the effect I have described, as the greater number of substances which will bear a sufficient heat, are fragile, oxidable, or affected by water, yet general considerations from the nearest analogies in chemistry would lead us to expect a similar effect from all matter in a state of intense ignition; even assuming the presence of solid matter to be necessary, the catalytic effects of platinum are shared in different degrees by other substances: it therefore appears probable that at a certain degree of heat water does not exist as water or steam, but is resolved into its constituent elements. If, therefore, there be planets whose physical condition is consistent with an intense heat, the probability is, that their atmosphere and the substances which compose them are in a totally different chemical state from ours, and resolved into what we call elements, but which by intense heat may be again resolved into more subtle elements. The same may be the case in the interior of our planet, subject however to the counter agency of pressure.

The experiments strongly tend to support the views of Bertholler, that chemical and physical attraction are affinal, or produced by the same mode of force. All calorific expansions appear to consist in a mechanical severance of the molecules of matter; and if heat produce effects of decomposition merely by increase of intensity, there seems no reason why we should assign to it in this case a different mode of action from its normal one. On this view physical division carried on indefinitely must ultimately produce decomposition, and chemical affinity is only another mode of molecular attraction. Thus a high degree of rarefaction, as at the bounds of the atmosphere, or in the interplanetary spaces, may entirely change the chemical condition of matter.

In a paper published in the Philosophical Transactions for 1843, p. 111, I have shown that we may oppose a chemical action by a physical one (electrolysis by a vacuum), that antagonizing chemical by physical tension, they mutually oppose each other. I believe the converse of this experiment has been made by M. Babinet, who by physical compression has prevented the development of chemical action.

I have also described in the Philosophical Magazine for November 1845, certain phenomena which appear to me to be irreconcileable with received chemical views; and though I then believed that the theory of Grotthus would be obliged to give way, I now incline to think that some of our chemical doctrines must ere long undergo a revision.

It is rather surprising that the valuable applications of which the phenomena of voltaic ignition are capable, and the fertile field which (as I believe) it presents for discoveries, both physical and chemical, should have been so completely neglected. It is true that until a recent period the imperfection of the voltaic battery rendered accurate and continued experiment on this subject difficult of performance, but still much might have been done. Davy made several experiments on the voltaic disrup-

tive discharge, which in many points may be regarded simply as very intense ignition; but I am only aware of two experiments of his on voltaic ignition; one, in which he employed it in an exhausted receiver to examine to what extent the radiation of heat was carried on *in vacuo*; and another, already alluded to, in which, by immersing a portion of an ignited wire in water, he observed that it conducted in some inverse ratio to its heat.

I have made a vast number of experiments on the voltaic arc or disruptive discharge, in various media*; when this is taken in a medium incapable of acting chemically on the electrodes, the phenomena are those of intense ignition of the terminals, which are dissipated in vapour and condensed upon the interior of the vessel in which the discharge is taken. I have examined some of these deposits, and they appear to consist of the metal of the terminals in a finely-divided state; this is strikingly shown with zinc. If the arc be taken between zinc points in an exhausted receiver, a fine dark powder, nearly black, is deposited on the interior, which, when collected, proves to be pure zinc, and on the application of a gentle heat, takes fire in the open air and burns into the white oxide: to casual observation the zinc would appear to be burned twice. The experiment appears to me to present an argument in favour of the dynamic theory of heat.

With charcoal, on the other hand, there is little or no deposit, but the charcoal continually yields carbonic oxide and hydrogen, and this for hours after the presence of water would be deemed impossible. I have taken the arc between pieces of well-burned charcoal for eight or nine successive hours, and there was still gas generated; indeed it appeared to be given off as long as there was any charcoal remaining, and a conversion of the carbon into inflammable gas might have been supposed. Much still remains to be done with this powerful agent, the voltaic arc: where, however, the object is simply to expose gases to an intense heat, the ignition of a conjunctive wire of platinum is more simple in its application, more uniform in its action, and instead of requiring a powerful battery, the effect can be satisfactorily produced by five or six cells, in many cases by two.

The heat is not so intense as that of the arc, but as it can be brought to within a few degrees of the fusing-point of platinum, it is far more intense than any heat usually employed in laboratories, certainly than any which can be applied to minute, I may say microscopic portions of gas or vapour.

In conclusion, I must express my sincere thanks to the managers of the London Institution, for having permitted me, as an honorary member, to carry on these experiments in the laboratory of the Institution.

* Philosophical Magazine, June 1840. Literary Gazette and Athenæum, February 7, 1845.

London Institution,
August 21, 1846.

II. *Supplementary Paper on certain Phenomena of Voltaic Ignition, and the Decomposition of Water into its Constituent Gases by Heat.* By W. R. Grove, *Esq.*

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In selecting the above title, I endeavoured to give as clear an enunciation of the phenomena to be described in the paper as was consistent with the brevity usual in a title.

An exception has, however, been taken to it, that as the effects of decomposition are produced by ignited platinum, the phenomena may result from that obscure mode of action called catalysis. That I did not intend to exclude from consideration any possible action of the substance employed, will be evident from the paper itself, in which I have called attention to the general production of catalytic effects by solid bodies.

Whatever value or novelty there may be in the facts I have communicated, is the same whether they be regarded as resulting from catalytic or from thermic actions. If the action be catalytic, it is one absolutely the reverse of that usually produced by platinum, and therefore just as much at variance with received experience as decomposition of water by heat would be; the effect of platinum, like that of heat, on the elements of water having been hitherto known only as combining them. With regard to any theoretic views I may have advanced, I by no means attach the same importance to them as I do to the facts themselves, though I consider it necessary for the collation of facts, and desirable for the progress of science, that an author pretending to communicate new results should give with them the impressions which led to their discovery, and the inferences which he regards as immediately deducible from them. No expression can be given to facts which does not involve some theory, and admitting the difficulty (perhaps insuperable) of correctly enunciating new phenomena, and the probability of future discoveries entirely changing our views regarding them, I cannot at present see that the title of my paper could be altered without being open to greater objections. I am of this opinion, not so much because other bodies than platinum will produce the effect, as I shall presently show, nor from the fact that the electrical spark will decompose aqueous vapour, though these are arguments in its favour; but from the following considerations. The catalytic action of platinum will induce or enable combination to take place where there is already a strong affinity or tendency to combine, as with mixed oxygen and hydrogen gases; it will also induce decomposition where the affinities are extremely weak, or in a state of unstable equilibrium, as in Thenard's peroxide of hydrogen; again, where there are

nicely-balanced compound affinities, it may change the chemical arrangement of the constituents of a compound, but I do not know of any case in which a powerful chemical affinity can be overcome by catalytic action; to effect this we require some natural force of greater intensity than that to be overcome. We might as well say that the platinum electrodes of a voltaic battery decompose water, as to say that platinum decomposes it in the case in question; there, the force of electricity acts only by means of matter, and matter of a peculiar description; its action also is only perceptible at the surface of this matter. I seek to use the expression in my title with reference to heat in a similar sense to that in which we use similar terms with reference to electricity, *i. e.* to regard heat as the immediate dynamic force which overcomes the affinity; thus, as we say when employing the voltaic battery, that we decompose water by electricity, so here we should say that we decompose it by heat.

If it be said that heat so weakens or antagonizes the affinity of the elements of water as to enable catalytic action to separate them, this amounts to the same theory, as heat is then regarded as the antagonizing force, and in this case the action, both thermic and catalytic, is the reverse of the normal action. I have thought it desirable shortly to discuss this question as likely to lead to further investigation, though I have been somewhat embarrassed by the want of definite meaning in the term catalysis; I must plead guilty to have frequently used the term, but notwithstanding, or perhaps on account of, its convenience, it has I fear had an injurious effect on scientific perspicuity.

The following experiments were made to ascertain whether platinum was the only substance by which the effect could be produced. A knob or button of the native alloy of iridium and osmium of the size of a small pea was formed by the voltaic battery; to this was attached by fusion another smaller knob of the same metal one-fourth the size of the former, and to this smaller one was attached a stout platinum wire; the object of the second knob was both to prevent the fusion of the platinum wire and also to avoid the possibility of any surface of platinum being exposed to the recipient tube or alloyed with the metal to be heated. The preparation of this simple instrument was very troublesome, but when made it answered the purpose well; the larger button could be fully ignited to an intense glow, while on account of the narrow neck which united them, the smaller was barely red-hot, and the platinum wire not perceptibly ignited. An experiment having been made with this metallic button and prepared water, similar to that previously made with platinum, gas was given off which averaged 0·3 of mixed gas; the residue was nitrogen mixed with varying small quantities of oxygen. The effect, upon the whole, was decidedly inferior to that of the platinum. Indeed as platinum is the most dense and unalterable of all known substances, it would be likely, upon any received theory of heat, to produce the greatest effects.

I tried palladium in the same manner; the gas yielded was hydrogen with small quantities of oxygen, and the water was stained with the oxide o the metal.

I now tried silica and other oxides, but the results were not very satisfactory. A spheroid of silica was formed by fusing pulverized silica on to a platinum wire, so as to cover it for the length of 0·4 of an inch; when this was plunged into the hot water and again fused in the oxyhydrogen blowpipe, it constantly became frothed with small bubbles of vapour, and after a few experiments generally separated in fissures; in the experiment which was continued for the longest time without disintegration, the gas given off contained 0·15 of oxyhydrogen gas; from the whole result I believe there is an action of the water on the silica (probably forming a hydrate decomposable by heat) which is a bar to satisfactory results. With other oxides, at least such as would bear an intense heat, the difficulties were still more insuperable. PRIESTLEY has shown that water will corrode glass, and if I mistake not, others have shown the same effect produced on silica.

Although, as applied to the facts detailed, I attached no further meaning to the title of my paper than that which I have above stated, yet in one or two theoretical inferences I have certainly gone further; for instance, when I suppose the possibility or probability of mechanical rarefaction producing the same effects as heat, here (although I do not, indeed I cannot conceive the existence of heat without matter) I certainly abstract from the proposition any consideration of solid matter. In order to ascertain how far this view might be founded on truth, I had thought of making a few experiments on the effect of mechanical rarefaction on the tendency of gases to combine, but (in addition to the interference of necessary occupations) I find that M. DE GROTTHUS has already experimented on the point; his experiments, as far as they go, corroborate the views I have put forth.

He finds* that mixed gases, such as chlorine and hydrogen, or oxygen and hydrogen, when rarefied either by slow increments of heat or by the air-pump, do not take fire ("ne s'enflamment pas") by the electric spark. From the context, he evidently means that the gases will not detonate or unite in volumes, as he states that a partial combination ensues. GROTTHUS appears to have considered the combination of gases by the electric spark as an effect of sudden compression or molecular approximation, certain particles being brought within the range of their affinities by the sudden dilatation of others. Although he did not pursue the subject far enough to ascertain whether a degree of rarefaction could be reached which would be an actual bar to combination, still his experiments strengthen those views which assimilate mechanical and thermic molecular repulsion, and regard chemical affinity as being antagonized by physical repulsion.

Pursuing the series of analogies from the decomposition of euchlorine at a low temperature, that of ammonia at a higher, that of metallic oxides at a higher, and so on to oxide of hydrogen, there appears to be an extensive series of facts which afford strong hope of a generalized antagonism between thermic repulsion and chemical

* Annales de Chimie, vol. lxxxii.

affinity, and a consequent establishment of the law of continuity in reference to physical and chemical attraction.

The deposit from chlorine, to which I have alluded in my paper, I have since examined, and though it differs in colour from that described in books, I find it is a protochloride of platinum, formed at the expense of the platinum wire. The larger portion of the chlorine in the tube combines with the hydrogen of the aqueous vapour, and the muriatic acid is absorbed by the water; when the experiment terminates the gaseous volume is reduced to nearly one-half, and this residue is oxygen.

This effect induced me to try an ignited wire on other analogues of chlorine, and I tried bromine and chloride of iodine in the apparatus (fig. 5). The tube was filled with the liquid, and its extremity was in the first experiments immersed in another narrow tube of the same liquid as that which filled it. When the platinum wire was ignited, permanent gas was given off both from the bromine and from the chloride of iodine, which gas on examination proved, to my surprise, to be oxygen. In one experiment I collected half a cubic inch of gas from an equal volume of chloride of iodine. As the experiment in this form required too large a quantity of the liquid to enable me to observe any change which might take place in its character, I repeated it with a tube five feet long, bent in two angular curves. A small quantity of the liquid was placed in the extremity of the tube containing the wire, which was so arranged as to be the lowest point; the angles were placed in cold water and the experiment proceeded with; my object was to enable the dense vapour of the liquids to shelter them from the atmosphere, there being no satisfactory method of shutting them in and yet allowing room for the elimination of the liberated gas, or of absorbing the latter by combination without also absorbing the vapours.

I had hoped by the above means to proceed with the experiments until all the oxygen was liberated that could be driven off, and then to have examined the residua; but I found that after experimenting for a short time, both the platinum wire and the glass in proximity to it were attacked by the liquids; this difficulty, similar to those which have hitherto prevented the isolation of fluorine, I have not yet been able to conquer, though I hope to resume the experiments.

As chloride of iodine is decomposed by water, it cannot contain any notable quantity of the latter, but, until the experiments are carried further, it must remain a question whether the oxygen results from a small quantity of water contained in the liquid, the hydrogen combining with the liquid itself, or from a decomposition similar to that of the peroxides. The experiments certainly add a new and striking analogy to those already known to exist between the peroxides and the halogens, but they do not, as far as I have hitherto carried them, necessarily prove analogy of composition.

In conclusion, I would call attention to a point which I omitted to notice in my original paper, viz. the explanation afforded by the results contained in it of the

hitherto mysterious phenomena of the non-polar decomposition of water by electrical discharges, as in the experiments of Pearson and Wollaston. This class of decompositions may now be carried much further. With the exception of fused metals, I know of no liquid, which, when exposed to intense heat such as that given by the electric spark, the voltaic arc, or incandescent platinum, does not give off permanent gas; phosphorus, sulphur, acids, hydrocarbons, water, salts, bromine and chloride of iodine, all yield gaseous matter.

Viewing these effects simply as facts, and without entering on any theoretical explanations or speculations, I cannot but think that there is a remarkable generality pertaining to them worthy of the most careful attention.

The apparatus I have described, particularly that represented by fig. 5, and the numerous applications of voltaic ignition which will occur to those who duly consider the subject, promise, I venture to believe, new methods and powers of investigating the molecular constitution of matter, and will, I trust, lead to novel and important results.

Nov. 10, 1846.

III. *Microscopic Observations on the so-called Vesicular Vapours of Water, as existing in the Vapours of Steam, and in Clouds, &c.* By A. Waller, *Esq., M.D.* *Communicated by* P. M. Roget, *M.D., Sec. R.S.*

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In a paper published in the Philosophical Magazine, February 1846, respecting some molecular actions of crystalline particles, I endeavoured to explain the fixation of particles of mercury, and the consequent formation of images in the Daguerreotype process. My experiments led me at that time to the conclusion, that the vapours, or rather fumes of condensed steam, consist of minute globules or spherules of water, and not of small vesicles according to the theory universally received at present, and expressed by the term of *vesicular vapour*.

My opinion on this point was founded on the results obtained by microscopic inspection of the condensed vapours of mercury and of other bodies, at the same time I stated that I was unable in the case of water to confirm it by direct observation. Subsequently, however, I have succeeded in doing so, and propose to show from the following observations that the opinions entertained respecting vesicular particles of water are completely erroneous. But before I describe my own experiments, I think it proper to state some of the ideas of our predecessors in science on vapours of water, and likewise the grounds on which they supported them.

Sir Isaac Newton, on various occasions in his Optics, mentions the particles composing clouds, fogs and mists, &c., so as to leave no doubt that he considered them to be composed of minute globules or spherules, as may be seen in the following passages:—"Between the parts of opaque and coloured bodies, are many spaces either empty or replenished with mediums of other densities; as water between the tinging corpuscles wherewith any liquor is impregnated, air between the *aqueous globules that constitute clouds and mists*."—Prop. III. Book 2nd. "But when in order to compose drops of rain they begin to coalesce and constitute globules of all intermediate sizes, these *globules*, when they become of a convenient size to reflect some colours and transmit others, may constitute clouds of various colours according to their sizes. And I see not what can be rationally conceived in so transparent a substance as water for the production of these colours, besides the various sizes of its fluid and *globular* particles."—Prop. V. In noticing the coloured areola surrounding the sun and moon in certain circumstances, he does not fail, when giving the measurement of their diameters, to mention the way in which they may be applied in obtaining a knowledge of the relative diameters of the globules of water which give rise to them.

M. Kaëmtz has obtained several interesting results in making use of this mode of observation, which has so long been neglected by meteorologists; by means of these rings he has ascertained that the average sizes of these particles vary in the different months of the year, and that in summer they are much smaller than in winter; the mean size he assigns to them is about $0{\cdot}0224^{mm}$.

The opinion now entertained respecting these particles of water, consists in regarding them as composed of small bladders or vesicles, similar in all respects, but their size, to the common soap-bubble. This theory is generally ascribed to Halley. Neither Newton nor Halley appears to have taken any steps to submit their hypotheses to direct observation. This was first attempted by Kratzenstein, whose experiments on this subject, dating from 1743, evince great ingenuity. The original work is very scarce in France, and not to be obtained in this country. But De Saussure, in his 'Traité sur l'Hygrométrie,' has cited them textually, from whence I extract the following. "M. Kratzenstein qui s'est beaucoup occupé de ces vésicules et qui a même prétendu réduire à elle seules tous les genres de vapeurs, a tenté de les mésurer et les a comparés avec un cheveu, et il a même cru pouvoir assurer que leur diamètre était douze fois plus petit. Le cheveu avait suivant M. K. $\frac{1}{300}$ de pouce et par conséquent ces vésicules une 3600 de la même mesure*."

Kratzenstein, after measuring the diameter of these supposed vesicular globules, attempted to determine the thickness of the pellicle which he believed to exist around them, and thus states his experiment in § 4 of his dissertation:—"J'ai pris un globe de verre qui avait cinq pouces de diamètre; à son orifice était adapté un robinet. En soufflant dans le globe j'ai comprimé l'air qui y était contenu, puis ayant fermé le robinet j'ai exposé le globe aux rayons du soleil dans la chambre obscure; mais je n'ai pu appercevoir aucune des vapeurs que j'avais fait entrer en y soufflant. Ayant ouvert le robinet pour faire sortir l'air comprimé, j'ai vu d'abord une grande quantité de vapeurs qui tombaient; mais elles ont encore disparu lorsque j'ai comprimé de nouveau l'air qui était dans le globe. Regardant de manière que mon œil fit avec le rayon du soleil un angle entre 5 et 10 degrés, j'ai apperçu avec un grand plaisir une suite de très belles couleurs qui se changaient peu à peu en d'autres à mesure que l'air comprimé sortait de la boule. Voici la suite de couleurs telle que je l'ai remarquée, rouge, verd, bleuâtre, rouge, verd. Ayant mis mon œil entre le soleil et les vapeurs et les ayant regardé sous les mêmes angles que je viens de dire, j'ai apperçu les mêmes couleurs que donnaient la réflexion, mais elles étaient dans un ordre inverse." Kratzenstein supposes that these colours arose from the expansion of the vesicles which caused their parietes to become thinner, and to assume colours in the same way as the soap-bubble; he calculated their thicknesses accordingly, which on an average he found to be about $0{\cdot}06^{mm}$. De Saussure, in quoting this experiment, states that it never succeeded in his hands, for the colours were simul-

* Théorie de l'élevation des vapeurs et des exhalaisons démontré mathématiquement, par M. Gottlieb Kratzenstein. Bordeaux, 1743.

taneous and not successive, as is required. These appearances are easily explained in the present state of science, which could not be done in the time of KRATZENSTEIN. The condensation of the vapours of water is caused by the sudden demand for caloric by the rarefied air in the glass globe, and the same takes place in the receiver of the pneumatic machine when there is a commencement of a vacuum. As the gas within recovers its caloric from surrounding bodies, the globular vapours and the moisture deposited on the inner surface of the glass recover their elastic condition. With regard to the colours observed, they are owing to the *diffraction of light* by the particles in suspension, and more particularly by those parts condensed on the sides of the glass, as in some of FRAUNHOFER's experiments. Therefore it is in no manner possible to connect these colours either with the thickness of the vesicles, or with the vesicular theory.

I have related these experiments thus fully, because I find that even at the present day, they still pass current with observers of a deservedly high reputation. Thus they are reproduced in KAEMTZ's Treatise on Meteorology, perhaps the most profound work on the science, and now rendered popular in this country by Mr. WALKER's translation of his Manual.

DE SAUSSURE, on examining with a lens of an inch focus the steam of water floating over a dark surface, in speaking of the vesicles, says, "La légéreté de ces petites sphères, leur blancheur, leur apparence absolument différente de celles des globules solides, leur parfaite ressemblance avec les bulles volumineuses que l'on voit nager à la surface du liquide ne laissent aucun doute sur leur nature; il suffit de les voir pour être convaincu que ce sont des sphères creuses, semblable à la grosseur près à celles que l'on forme avec l'eau de savon." He further states that he confirmed these ideas by the examination of fogs or clouds on high mountains, when he was enabled to perceive in the same way minute vesicles, sometimes accompanied with drops or globules of water. He estimates the smallest at $\frac{1}{1800}$th of an inch, and the largest at $\frac{1}{3780}$th. When these vesicles came in contact, they burst and formed a small drop of water. These experiments have been adopted and reproduced by most modern authors, among whom may be mentioned BERZELIUS, FRESNEL, MITSCHERLICH, &c.

As I have already stated in the Philosophical Magazine, I had an opportunity of repeating DE SAUSSURE's observations on the clouds, at the Monastery of St. Bernard. I will quote the passage: "Globules of various sizes are frequently discerned by the naked eye floating in all directions. I have endeavoured to ascertain their vesicular structure, but have been unable to do so from direct observations. It is frequently a most difficult point in microscopic investigation to decide upon the existence of a thin transparent membrane. It is still more so to pronounce upon the vesicular or spherular structure of globules in constant agitation; and I believe that if minute spherules and vesicles could be mixed together, we do not possess any means at present of distinguishing them. I have never been able to detect that appearance

of bursting of the globules mentioned by De Saussure, but sometimes when the agitation of the air is slight, two of the larger globules may be seen floating towards each other, and afterwards disappear suddenly, which may be explained, if we admit that it is caused by the union of the two spherules into one, which is too heavy to remain any longer in suspension, and whose rapid deposition conceals it from the sight." From the commencement of my experiments, I found that the greatest obstacle to a perfect investigation of these globules arose from their excessive mobility. Even to the naked eye this is very great, and is the cause of several optical delusions; but by the use of magnifying instruments, this mobility must necessarily be increased in proportion to their power. With the simple microscope of De Saussure, a globule might occasionally be perceived, and its diameter estimated to a certain extent; but with the compound achromatic microscope I employ, their angular displacement is too rapid to form any correct sensation on the retina. For this reason I was led to adopt the plan of fixing the condensed vapours arising from the breath or other sources, in some liquid which, like oil, possesses no affinity for water. The liquids with which I thus mixed the vapours of water were very numerous; and as the appearances thus presented under the microscope were not the same in all cases, I will only mention some of the most interesting.

Canada balsam is, perhaps, of all other vehicles, the most adapted for these observations. A slip of glass covered with a thin layer of it is to be used. By breathing on it with a little force, the vapours of the breath will not only be condensed on its surface, but will penetrate below, where they may be easily recognized in opake streaks of a white colour. By reason of the viscousness of the balsam, they will remain almost stationary for more than an hour, and for a much longer period if covered with another thin piece of glass or talc. Under the microscope these streaks are decomposed into minute globules perfectly spherular like shot, or the globules of mercury. See Plate II. fig. 1. *bb*. Their sizes vary within certain limits, between which they occur, of all intermediate sizes. They agglomerate together in various ways, forming lines composed of several rows of them touching each other, but without any signs of coalescing, unless attentively examined for some time, when two smaller ones may be perceived to disappear, and in their place is seen another of larger size resulting from their fusion. A certain degree of attraction exists between them, as they are generally packed together as closely as possible, and any hiatus caused by any of them coalescing is quickly filled by others. Thus a group will sometimes have its form modified and reduced by their pressing inwards to supply any hiatus left after one or two have coalesced. Another proof of their mutual attraction is offered in the movement *en masse* of a single file of globules without their separating from each other. In favourable circumstances, I have found some of them still in "statu quo" after a lapse of twenty-four hours or more. Their diameters vary generally from $0{\cdot}001^{mm}$ to $0{\cdot}003^{mm}$. They disappear at length by adhering to and wetting the surface of the glass above or below them, where they may easily escape notice, as from

their flattened surfaces they possess but little power to deviate the light from its regular course. Their shape is very irregular, generally of an angular, and frequently of an hexagonal form. See fig. 1. *aa*. That part condensed on the surface of the balsam presents exceedingly minute globules of the same nature.

Instead of condensing the breath as above, I have placed the slip of glass covered with balsam in the open air, on grass covered with hoar-frost, and after it had become slightly turbid on its surface from the moisture deposited, I submitted it to the microscope. It was then seen to be covered with lines of single globules like strung beads, following the most tortuous courses; and wherever the balsam had been disturbed by the passage of any body through it even for a long time previous to the exposure, the direction of these lines showed the manner in which it had been disturbed. The diameters of some of these globules appeared as mere points under the strongest magnifying power, and were probably less than $0{\cdot}0001^{mm}$. As the glass became warm the globules coalesced at intermediate points into a single one, which soon vanished in its turn. When steam from boiling water in a glass tube was condensed in Canada balsam, the globules which are represented in fig. 2 are much more voluminous, varying from $0{\cdot}01^{mm}$ to $0{\cdot}015^{mm}$, though in other respects they were exactly the same. The globules arising from water at 60° Reaumur are the same size as those of the breath.

No doubt can possibly be entertained, after having inspected these globules attentively, of their being perfect spherules of liquid without any central part whatever, for containing a gasiform fluid, which could not escape detection when they coalesce and adhere to the glass. Globules of gas, of whatever nature, cannot possibly be confounded with one of these liquid globules, from their being much darker, and the smallest of them considerably larger than those of water, independently of their remaining permanent when inclosed between two surfaces of glass. In Canada balsam they present, on a very minute scale, the exact representation of external objects which surround the microscope, which, however, is seen on liquid globules when they are sufficiently large. Thus, in looking through the microscope, I have seen at the lower part of one of these air-globules portions of my own face or body; and in the same way an extensive view of trees, houses, &c. may be traced with all its details on one of these microscopic globules not larger than $0{\cdot}01^{mm}$ or $0{\cdot}02^{mm}$. Fig. 3 represents the image of part of the hand, as seen in a globule of air, drawn under the camera lucida. I mention this fact, as I believe that the telescopic action of the microscope, by means of these hollow prisms, may be capable of several interesting applications.

By breathing gently on the surface of Canada balsam, it will become covered with a film of moisture, reflecting various colours in proportion to the quantity deposited. Any bright object viewed through it will appear surrounded by a halo of brilliant colours, such as those seen sometimes encircling the sun or moon. These coloured films, under the microscope, are decomposed into colourless particles of water, which

are all of the same form, although an irregular one, not completely circular. If a surface of balsam be placed aside for a short time and afterwards examined, we find that these particles, which before were disposed in a regular manner, appear to have altered their position, and that they have grouped themselves in radiating lines towards a variety of points. See fig. 4.

Particles of water may be examined in a solid and crystalline state. A slip of glass or a small glass tray, coated with Canada balsam on its under surface, and a freezing mixture at the upper, will gradually condense the moisture from the air, and the balsam will become covered with the well-known spongy deposit of frozen particles. The manner in which the crystallization takes place is remarkable. At first the moisture condensed is liquid and globular in form, but as the refrigeration continues a sudden molecular change occurs, and the globules are found to assume various crystalline shapes. When they are small and numerous, on consolidating, they form a kind of areolar net-work, the appearance of which is caused by the reflexion of numerous crystalline facets (see fig. 7); at other places may be seen small pyramidal crystals covering a solid globular nucleus like the head of a mace; sometimes they assume the form of small acicular, of hexagonal prisms with various secondary facets, of octohedrons, &c. At the moment when this molecular action takes place, I have sometimes perceived with the microscope a kind of flitting movement, as the globules are assuming their various shapes, like that which occurs in the crystallization of particles from a state of solution*.

The globules which compose mists or fogs may be condensed by exposing a slip of glass coated with balsam in the open air, but as the deposition takes place very slowly, it is preferable to employ the rotatory bellows, so placed as to throw the current of air at an angle of about 60° on the plate. Steam or other volatile substances may be condensed in the same manner. When the current is created by the common bellows, I have found it much more difficult to fix the globules. Those obtained by this process are exactly similar to those of steam. In my experiments they were between 0·02^{mm} and 0·03^{mm} in size.

Essence of turpentine may be used in the same manner as Canada balsam. The globules condensed in this manifest much greater mobility, as might be expected from the nature of the liquid, frequently rebounding from each other as they come in contact. They disappear very rapidly, either by adhering to the glass, or by coalescing together. I have observed them moving in vortices around several central points where they rapidly collapsed. Their diameters vary between the

* The most favourable season for performing these experiments is the winter. In summer, when the air is warm and loaded with moisture, the particles on condensing, form liquid or solid globules, or very confused crystals, and whenever the freezing mixture is moved aside, so as to expose a portion of the under surface to microscopic inspection, a very few moments suffice for the particles to liquefy. Solid globules may be distinguished from liquid ones by their borders being much darker, and from their frequently containing smaller globules, as in fig. 8.

0·001^{mm} and the 0·003^{mm}. Essence of turpentine is still better adapted than the balsam for retaining the globules suspended in the air, as in fogs, &c. When, after having been cooled below the common temperature, it is breathed upon, a whitish precipitate of a streaky appearance is formed like an insoluble salt, which slowly descends to the bottom of the vessel, where it coalesces into larger globules*. This is also the case with water-globules condensed in this way from the air, which form a cloudy precipitate intercepting the passage of light. On being preserved in a bottle this cloud will still remain after the lapse of a couple of days, when the water will be found at the bottom of the vessel in a globular shape like metallic mercury, without adhering to the sides of the glass. By means of this permanent cloud, we might be enabled to perform a series of experiments of great interest to meteorology. In this way we might determine the influence of water-globules of various sizes in the passage of luminous, thermic, and chemical rays.

Copaiva balsam, upon being breathed upon, becomes coated with a milky film, which, to the naked eye, assumes a reticulated appearance after a few moments, which is caused by the disappearance of the film sooner at some points than at others. Under the microscope the uniform film is decomposed into minute globules, which occupy the space of mere points under the strongest magnifying power. They disappear very rapidly by agglomerating and forming larger globules. This process of agglomeration commences simultaneously on the surface of the liquid at a variety of different points. From these it proceeds to form circular areas, which continue to extend until their line of demarcation consists of merely a single row of globules, which also in their turn contract and coalesce into small groups of about a dozen separate spherules. At this time the process of fusion ceases, and the globules are seen suddenly to fly apart in virtue of some molecular repulsion, and then gradually disappear. With boiling water the like phenomena are produced, except that the globules are all much larger. In fig. 5 are seen these actions in all their different stages.

Oil of peppermint condenses moisture which presents phenomena very similar to those of copaiva balsam. If the breath be passed through a narrow tube, a system of rays is produced, composed of separate globules, which remind one of the form of some of the species of Asterias which radiate from a centre, and are likewise composed of a multitude of separate globular pieces. The irisated colours formed on this oil are very brilliant, but soon disappear unless the current of air is constantly kept up. The particles of water which give rise to these colours, disappear by immersion into the oil, as well as with all the other oils which are susceptible of presenting them. The oils of anethum, carraway and cajeput, also present very similar appearances.

* A bottle containing condensed water-globules and essence of turpentine, exposed night and day in a northern aspect, became coated at its inner surface on the north side with condensed moisture, and around this with transparent acicular crystals arranged in zones around the moisture. The crystals were not obtained in sufficient quantity to admit of their being analysed.

Creosote condenses the breath, which agglomerates into globules apparent to the naked eye and long remains unaltered.

Olive oil.—The globules are very minute and agglomerate slowly. The surface assumes a lacerated appearance, and the molecular dispersion appears simultaneously over the entire field of vision. Croton oil condenses a film reflecting the various spectral colours.

Castor oil.—The condensed particles of water present the same appearance to the naked eye, or when assisted by the microscope, as the two preceding. If, before the globules have disappeared, they are covered over with another slip of glass having a film of oil previously placed on it, they are retained for a much longer period than if the surface of the glass had been perfectly dry. Almost all of them are adherent together, as in fig. 6, but without agglomerating into one. In some instances a larger globule, like a central nucleus, is seen covered with others adhering to it. Their appearance reminds one strongly of the manner in which the condensed vapours of sulphur are found adhering together.

Another means of subjecting particles of water to microscopic observation, consists in fixing them upon minute filaments, such as those of the spider's web. I have found it most convenient to employ the filaments which contain the ova of the spider, or those of the cocoon of the silk-worm. Thin bundles of either of these exposed to steam from boiling water, were found after a few moments to have very minute globules condensed upon them, mostly imperceptible to the naked eye, about the same size as those obtained in Canada balsam from boiling water. These particles of water were all liquid globules without any signs of vesicular structure. In foggy weather the filaments of the spider's web generally become covered with small globules of water, which in some cases are so minute as to give the filament a grey whitish aspect, like globules floating in the air, or in spirit of turpentine. These filaments, fixed between two small frames pressed close together, may be examined under the microscope. They are then seen covered with globules of water, and with others of an organic nature secreted by the spider. The former may readily be distinguished, by their quick evaporation, from the others which are permanent. The globules of water were about $0{\cdot}02^{mm}$.

We may therefore conclude from the foregoing observations, that the term vesicular structure of globules, which was first proposed at a time when the knowledge of gaseous bodies was still in its infancy, has been adopted without sufficient foundation; that the experiments on which it was grounded were unsatisfactory and imperfect; and that whenever we are enabled to inspect the minutest particles of water arising from condensed steam or vapour, they consist of minute liquid globules without any appearance of internal cavity.

Kensington, April 12th, 1846.

IV. *Quelques recherches sur l'Arc Voltaique, et sur l'influence qu'exerce le Magnétisme soit sur cet arc soit sur les corps qui transmettent les Courants Electriques Discontinus.*

Researches on the Voltaic Arc, and on the influence which Magnetism exerts both on this Arc and on bodies transmitting interrupted Electric Currents. By M. Auguste De la Rive, *Professor in the Academy of Geneva, Foreign Member of the Royal Society, Corresponding Member of the Academy of Sciences at Paris, &c. &c.*

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THE luminous voltaic arc occurring between two conducting bodies, each communicating with one of the poles of the pile, is not merely one of the most brilliant phenomena in physics, but, from the numerous aspects under which it may be regarded, it is also one of the most important.

As a source of light, this phenomenon, when exhibited in a vacuum, enables us to examine what influence this particular origin of the light employed may have in various optical experiments. Compared with the solar light, the light of the voltaic arc presents some curious differences and also resemblances. If, on the one hand, we find in it the seven coloured rays of the spectrum, on the other the black streaks are replaced by brilliant ones, and these are differently interspaced. In this field of inquiry, much, or rather all, yet remains to be investigated.

As a source of heat, the voltaic arc enables us to study the fusion and solidification of even the most refractory bodies *in vacuo*, and consequently under circumstances exempting them from oxidizing action and other chemical influences, which usually result from the application of a high temperature in atmospheric air. It likewise allows us to determine the effects produced upon bodies at a high temperature, by various gases or vapours, distinct from those which enter into the composition of atmospheric air, and at different degrees of density.

As an electro-chemical power, the voltaic arc may be applied so as to submit to the electrolizing action of the electric current gaseous media, which, from some experiments already made, appear capable of decomposition by this process.

As a mechanical power, the voltaic arc, by bringing bodies into a state of minute division, and impressing upon them, in this state, a tendency to motion, places them in a favourable condition for the study of their molecular constitution, and of the relations which connect this constitution with electricity and magnetism. The struggle that takes place between cohesion and the expansive force of the electric

current, the reduction of matter to the molecular state, and the form and nature of the deposits resulting therefrom, are so many phenomena capable of throwing light on the obscure subject of molecular physics.

The few preceding remarks suffice to give some idea of the extent of an investigation embracing the whole range of experimental research on the voltaic arc under its various aspects, which I am far from pretending to have attempted. I shall confine myself at present to a few details, and especially to such as exhibit the action of magnetism on the voltaic arc, and on those bodies which transmit interrupted currents. I shall begin by describing some particular phenomena which I observed during my study of the voltaic arc under various circumstances, while employing different substances as electrodes, both in the air and in a vacuum; I shall then proceed to examine the action of a powerful electro-magnet on this voltaic arc, and I shall conclude by describing some remarkable experiments also illustrating the influence of magnetism on conducting bodies, of whatever nature, traversed by interrupted currents.

§ 1. *Some Phenomena concerning the Voltaic Arc.*

Davy was the first who produced the phenomenon of the voltaic arc with two points of charcoal. More recently, Messrs. Grove* and Daniell† employed with success the points of different metals, and arrived at interesting results: I also published some experiments I made on the voltaic arc‡ in 1841. Subsequently, MM. Fizeau and Foucault observed some remarkable facts of the same kind on the occasion of an investigation into the intensity of the light emitted by charcoal in the experiment of Davy§. The researches made up to the present time, have already led to many results, of which I shall consider only the most important.

1. That the voltaic arc may be produced, a pile of greater tension is required than that which is necessary for the ordinary calorific and electro-chemical phenomena. The necessity of this condition proves the great resistance presented to the passage of the electric current by the minutely divided matter, whatever it may be, which connects the two poles.

2. The luminous arc cannot exist, unless contact be previously made between the electrodes, and unless these, or at least one of them, be terminated at the point of contact by points fine enough to produce in them an increase of temperature. When this increased temperature is once produced, we may, by separating the electrodes gradually and with precaution from each other, obtain the luminous arc, the length of which will depend on the intensity of the pile. Daniell discovered the important fact, which was confirmed by M. Van Breda in a very recent investigation inserted in the Comptes Rendus de l'Académie, that without contact having taken place, the luminous arc may be produced between two electrodes placed very near together, by causing the discharge of a Leyden jar to pass between them: this is owing to the

* Bibl. Univ. June 1840, i. 27. p. 387.
† Arch. de l'Elect. tom. i. p. 462.
‡ Arch. de l'Elect. tom. i. p. 262.
§ Ibid. tom. iv. p. 311.

discharge being always attended by the transference of highly diffused matter, which closes the circuit during the instant of time necessary for the formation of the arc.

3. The enormous elevation of temperature which accompanies the production of the luminous arc, is also manifested in the electrodes, especially in the positive ones, which become much more strongly heated than the negative.

4. Matter is thus transported from the positive electrode to the negative, a fact which may be verified with electrodes of all kinds, but particularly with those of charcoal.

5. The various phenomena presented by the voltaic arc, are modified to a greater or less extent by the nature of the electrodes and by that of the surrounding medium. Thus Mr. Grove adduces facts from which it appears that the presence of oxygen is necessary in most cases to produce a very luminous and brilliant arc. It results also from his experiments, as well as those of other philosophers, that when two different substances are made use of for the electrodes, it is not a matter of indifference which of the two is placed at the positive pole.

I now proceed to my own researches. I commenced by studying the production of a luminous arc between a plate and a point of the same material in air, and *in vacuo*. By means of a micrometer screw I was able to make the point recede from the plate very gradually, and judge of their mutual distance with great precision. The limit of distance beyond which the luminous arc ceases to appear, is constant for the same plate and the same point: when, however, the plate communicates with the positive pole, it is in general double that which it is when the point communicates with the same pole. But in proportion as the strength of the pile is greater, the difference is so much the smaller.

With respect to the absolute amount of this distance, it is very variable, depending on the strength of the pile, on the nature and molecular state of the electrodes, and on the time occupied in the experiment. Thus, with a Grove battery composed of fifty pairs of plates sixteen square inches in surface, it is two or three times greater than with a pile of seventy elements of two or three square inches. With metals easily fused or oxidized, as zinc and iron, it is much greater than with platinum or silver. The duration of the phenomenon influences the result, inasmuch as the high temperature of the electrodes allows them to be drawn asunder to a greater distance without breaking the arc. The same effect may be produced by heating them artificially, by means of a spirit-lamp. It is evident from what I have said that the length of the luminous arc has a relation to the greater or less facility which the substances composing the electrodes possess of being segregated, a facility which may depend upon their temperature diminishing their cohesion, upon their tendency to oxidize (which produces the same effect), upon their molecular state, and lastly upon their peculiar nature. Carbon derives from its molecular constitution, which renders it so friable, the property of being one of the substances which produces the longest luminous arc.

The deposits of the transported matter, form upon the plate, when it is negative and the point positive, a species of very regular ring, the centre of which is the projection of the point upon the plate. This takes place equally, whether the plate be vertical or horizontal, plainly indicating a determinate direction in the transfer of the substance from the positive to the negative electrode; in the air and with metallic electrodes, the deposits always consist of the oxidized dust of the metal, of which the positive electrode is composed.

I shall here enter into some details. A plate and a point of platinum have been used as electrodes in a vacuum, in air and in hydrogen. In a vacuum with a GROVE battery of fifty pairs of plates, which had previously been used, I had only a very feeble effect, and particularly when the plate served as the positive electrode. The point was hardly removed a millimetre* from the plate when the arc broke; to re-establish it, it became necessary to renew the contact between the point and the plate, by touching another point of the plate, the first point which was touched appearing to have undergone such a modification as to prevent the re-formation of the arc. The same effect is produced when the experiments are made in the air, but it ceases when the power of the battery is increased: this is probably due to an augmentation of cohesion consequent on the increase of temperature in that part of the plate which acts as the positive electrode. Besides, when the experiment is made in air, the voltaic arc is more marked and of greater length than when it is made *in vacuo*, at least if the battery be weak; for when the battery is powerful, composed, for example, of fifty pairs of plates freshly charged, it appeared to me that the contrary obtained. I did not, however, perceive any great difference; but the vacuum in which I experimented was far from being perfect; it was that of a pneumatic pump, enclosing therefore highly rarefied air.

In the latter case, that is to say, with the pile composed of fifty pairs strongly charged, and in highly rarefied air, a bluish spot, perfectly circular and presenting the appearance of a coloured ring of NOBILI, was formed on the plate of platinum when it served as the positive electrode. The same spot appeared in atmospheric air, but its diameter was one-half less, and its colours much less vivid. In hydrogen, no coloured spot was formed; its formation is therefore evidently the result of the oxidation of the platinum at a high temperature when acting as a positive electrode in the ordinary atmosphere, and still more so, perhaps, in rarefied air†. When the same plate of platinum was made use of as a negative electrode, the point being positive, it became covered with a white circular spot, formed of a vast number

* 1 millimetre = 0·03937 inch.—TRANS.

† This effect may possibly have been owing to the action of the oxygen brought by the voltaic current into that particular state which SCHÖNBEIN first described under the name of *ozone*. Indeed, in this state the oxygen may attack those metals which are supposed to be inoxidizable; and M. MARIGNAC and I have shown that this may be effected by causing a succession of electric discharges to pass through the oxygen, even when very dry, with which the phenomenon of the voltaic arc has a great resemblance.

of minute grains of platinum, which, having been raised to a high temperature, remained adhering to the surface. The white spot, like the blue one, was much larger in rarefied air than in a vacuum. If the experiment be prolonged for a minute or two when the plate is negative, the rod of platinum terminating in a point, which is positive, soon becomes highly incandescent; its end is fused and falls on the plate in the form of a perfectly spherical globule. When the plate is positive and the point negative, the latter is less heated, and does not become fused; but the plate, unless it be very thick, is liable to be perforated: besides, as may easily be imagined, the phenomenon lasts much longer in the latter case. The light is less brilliant, but it is accompanied by a reflexion of a superb blue, which may be seen when the experiment is made in the interior of a bell, whether the air be rarefied or not. This blue reflexion is observed on the side of the bell, and is to be seen whatever may be the nature of the electrodes, or the colour of the light to which these give rise in the centre of the bell; only when this central light is very brilliant, it becomes slightly paler by the effect of contrast.

I substituted for the platinum point a point of coke, but the plate of platinum remained; this being positive and the point negative, I obtained a luminous arc more than double the length of the arc produced by the point of platinum. With respect to the arc, instead of its being a cone of light, having its base on the plate and its apex at the point, as was the case when the latter was platinum, it was composed of a multitude of luminous jets diverging from different points of the plate, and tending to various parts of the point of coke. This fact shows clearly the influence that may be exercised by the negative electrode, the function of which is very far from being a merely passive one. Let me add, that although the strength of the pile was precisely the same as when the point was of platinum, not only was the luminous arc much longer with the point of coke, but the heat developed in the plate of platinum was so much greater that it was soon melted and perforated. The coke being positive and the plate negative, the length of the arc was less than in the preceding case, and particularly so in air, where it was sensibly less than in a vacuum. The heat generated was however still very great, the point of coke becoming quickly incandescent throughout. I ought to add, that with the point of coke, the luminous arc was so brilliant that the blue light which I have mentioned almost entirely disappeared, which was not the case with any other kind of point.

Leaving the plate of platinum, I adjusted a zinc point. The effects were most brilliant, but of short duration, the point speedily melting. In common air, a deposit of white oxide was precipitated upon the platinum plate; in highly rarefied air (the vacuum of an air-pump), a black deposit was formed: in both cases it communicated with the positive pole. An iron point being substituted for that of zinc, equally produced in common air a brownish red deposit of oxide of iron, and in rarefied air a deposit of black oxide.

I call the attention of chemists to these two facts, as well as that of the oxidation

of the platinum at a high temperature in rarefied air. They appear to prove the influence which the state of greater or less density of the surrounding oxygen may exert on the phenomenon of oxidation and on the nature of the oxide formed. A plate and a point of soft iron were used as positive and negative electrodes, both in a vacuum and in the atmosphere; the same results appeared with a plate and a point of silver, a plate and a point of copper, and a plate and point of argentane*. The blue light was perceived in all the experiments; coloured circles were likewise seen on all the plates when they had acted as positive electrodes in rarefied air. The silver and copper plates presented in this case very decided cavities, caused by the passage of the matter from the positive to the negative pole. The points became incandescent throughout when they served as positive electrodes; whereas when negative, they were heated only at their extremities. The copper point when positive became isolating at its extremity, and it was necessary to excite it by friction in order to renew the experiment. This circumstance is probably attributable to the formation of a thin film of oxide. The point and plate of copper gave out a luminous arc of a beautiful green light, which contrasted in a remarkable manner with the blue reflexion visible in this, as in the other experiments. Mercury was likewise employed, both as a positive and negative electrode. In a vacuum as well as in atmospheric air, the luminous effect was most brilliant. The mercury was excessively agitated, rising up in the form of a cone when it was positive, and sinking considerably below the positive point when it was negative. The quantity of vapour thrown off by the mercury during this experiment filled the bell so quickly that it was not easy to observe the details.

I shall terminate this section by stating a fact which appears to me to be important; it is the influence which the nature of the metallic points forming the electrodes exercises on the temperature which they acquire in relation to the production of the voltaic arc. If the two points are of the same metal, both platinum, or both silver, the positive one alone becomes incandescent throughout its whole length. If the silver point be positive and that of the platinum negative, the latter becomes incandescent, and the silver one is much less heated. Thus, when the voltaic arc is formed, the circuit must be regarded as completed, and then it is those parts of the circuit which present the greatest resistance to the current which become the hottest; at first it is that portion forming the arc itself, and then, in the rest of the circuit, the metal which is the worst conductor. But if the conductors be of the same material on both sides of the arc, or if there be only a slight difference of conductibility between them, then the development of heat, instead of being uniform, as it might appear it ought to be, is much greater on the positive side. This important fact evidently proves that this portion of the circuit has to resist a much more energetic action than that which the other side experiences; a fact which is confirmed by the molecular segregation accompanying this action at the positive electrode. This want

* An alloy of copper and nickel: also known by the names of *packfong* and *melchior*.

of resemblance in the phenomena presented by the two electrodes, although placed in conditions entirely symmetrical, deserves to be taken into serious consideration, for it may throw light upon the nature of the electric current, and upon the link which unites it with the molecular state of the bodies through which it is transmitted.

§ 2. *Influence of Magnetism on the Voltaic Arc.*

Davy was the first who observed that a powerful magnet acts upon the voltaic arc as upon a moveable conductor, traversed by an electric current; it attracts and repels it, and this repulsion and attraction manifests itself by a change in the form of the arc. Even the action of the magnet may, as I have found, break the arc by too great an attraction or repulsion exerted upon it, causing the communication which the transmitted particles establish between the electrodes to cease.

The action which I have just mentioned is not the only one which magnetism exerts on the voltaic arc. I have already stated the curious fact, that if two points of soft iron acting as electrodes, be both placed within a helix formed of thick copper wire of several coils, the voltaic arc developed between the two points of iron ceases the moment a strong current is passed through the wire of the helices, and reappears if this current be arrested before the points have become cold. The arc cannot be formed between the two iron points when they are magnetized, whether by the action of the helices, or by that of a powerful magnet, unless they be brought much nearer to one another, and the appearance of the phenomenon is then entirely different. The transported particles appear to disengage themselves with difficulty from the positive electrode, sparks fly with noise in all directions, while in the former case, it was a vivid light without sparks, and without noise, accompanied by the transfer of a liquid mass, and this appeared to be effected with the greatest ease. It is of little moment with respect to the result of the experiment, whether the two rods of magnetized iron present to that part of their extremities between which the luminous arc springs, the same magnetic poles or different poles.

The positive electrode of iron, when it is strongly magnetized, produces, the moment that the voltaic arc is formed between it and a negative electrode of whatever nature, a very intense noise, analogous to the sharp hissing sound of steam issuing from a locomotive engine. This noise ceases simultaneously with the magnetization.

For the purpose of better analysing these different phenomena, I placed an electro-magnet of large dimensions and great power in such a manner as to enable me to place on each of its poles, or between them, different metals destined to form one of the electrodes of the pile, while one point of the same metal, or another substance, acted as the other electrode. I have alike employed as electrodes, placing them in the same circumstances, two points of the same metal or of different metals. The following are the results which I have obtained. A plate of platinum was placed on one of the poles of the electro-magnet, and a point of the same metal was placed vertically above it; the voltaic arc was produced between the plate and the point, the

plate being positive and the point negative. As soon as the electro-magnet was charged, a sharp hissing was heard; it became necessary to bring the point of the plate nearer to enable the arc to continue, and the bluish circular spot which the platinum plate presented, became larger than when the experiment was made beyond the influence of the electro-magnet. The plate was made negative, and the point positive; the effect was then totally different; the luminous arc no longer maintained its vertical direction when the electro-magnet was charged, but took an oblique direction, as if it had been projected outwards towards the margin of the plate; it was broken incessantly, each time accompanied by a sharp and sudden noise, similar to the discharge of a Leyden jar. The direction in which the luminous arc is projected, depends upon the direction of the current producing it, as likewise on the position of the plate on one or other of the two poles, or between the poles of the electro-magnet. A plate and a point of silver, a plate and a point of copper, and generally a plate and a point of any other metal, provided it be not metal too easily fused, present the same phenomena.

Copper, and still more silver, present a remarkable peculiarity. Plates of these two metals retain on their surfaces the impression of the action that took place in the experiments just described. Thus, when the plate is positive, that portion of its surface lying beneath the negative point presents a spot in the form of a helix; as if the metal melted in this locality had undergone a gyratory motion around a centre, at the same time that it was uplifted in the shape of a cone towards the point. Moreover, the curve of the helix is fringed throughout by minute ramifications, precisely similar to the tufts which mark the passage of positive electricity in a Leyden jar. When the plate is negative and the point positive, the marks are totally different, being merely a simple point, or rather a circle of a very small diameter, whence proceeds a line more or less curved, forming a kind of tail to the comet, of which the small circle might be the nucleus: the direction of this tail depends upon the direction in which the luminous arc has been projected.

When, instead of a plate and a point, two points are used for electrodes, it is evident that no visible trace of this phenomenon can be obtained; but both the sharp hissing and the detonations may be produced, which latter are sometimes so loud as to bear a resemblance to distant discharges of musketry. For this the electro-magnet must be very powerful, and the current which produces the arc very intense. I had observed that when I took for a positive electrode a point of platinum, and for a negative electrode a point of copper, and placed them between the two poles of the electro-magnet, the production of the voltaic arc between the two poles was accompanied by a sharp hissing noise; whereas in the opposite case, the copper being positive, and the platinum negative, the detonations were heard, attended by a frequent breaking of the arc. On examining this phenomenon more closely, I perceived that the fact I have just mentioned was due to the platinum becoming heated much more rapidly than the copper when they were employed as electrodes in producing the voltaic arc; and

I have satisfied myself that in order to obtain the hissing sounds, it is necessary that the positive electrode should be at a sufficiently high temperature to experience a commencement of liquefaction; for without this condition, only a series of detonations are heard. The hissing would be the result of the easy and continuous transport of matter more or less liquefied from the positive electrode, whilst the detonations would be the effect of the resistance opposed by the same matter to the disintegration of its particles when it is not sufficiently heated. Numerous experiments made with metallic points, whether of the same or different natures, as silver, iron, brass, as also platinum and copper, some of which become heated sooner than others under the same circumstances, have quite confirmed me in this view of the subject. It is merely necessary to be careful, in order to produce the hissing noise, to maintain as much as possible the continuity of the arc when once the positive electrode becomes incandescent; while, on the other hand, to obtain the detonations, one of the electrodes must be held in the hand, and then the arc frequently made and broken without waiting till the metallic points acquire too high a temperature.

It remains now to be considered why the influence of powerful magnetism, such as that exerted by the electro-magnet, is necessary for the production of these sounds, which are not heard in the ordinary experiment of the voltaic arc. This can arise only from the change which the magnet produces in the molecular constitution of the matter of the electrode, or rather in the highly diffused matter which forms the voltaic arc. This action is besides shown by the shortening of the arc, and by the remarkable difference which it presents in its appearance; it is therefore not surprising that it should also be capable of producing a phenomenon such as sound, which essentially depends on the variations in the molecular state of bodies. This view of the subject appears to me to deserve very particular attention: the results at which I have arrived, in pursuing it more closely, form the subject of the following section.

§ 3. *Influence of the permanent action of Magnetism on conducting bodies traversed by interrupted electric currents.*

Faraday's brilliant discovery of the action exerted by magnetism on a ray of polarized light, when that ray traverses a transparent body submitted to the action of a powerful electro-magnet, had no sooner been announced by its illustrious author, than the majority of philosophers saw in it a proof that magnetism, when at a high degree of intensity, has power to modify the molecular constitution of all bodies. They consequently attributed the phenomenon observed by Faraday, not to the direct action of the electro-magnet on the polarized ray, but to the modification effected by this action on the molecular constitution of the substance traversed by the ray. I was of this opinion, and communicated it to Mr. Faraday, who alludes to it in his memoir. Desirous, however, of founding this opinion on facts of a different kind, I asked myself if it were not possible to find in the electric current, an agent capable of performing the same function for opake conducting bodies that polarized light does for

transparent ones. I had stated in my paper on the sound emitted by iron wires traversed by interrupted electric currents, that the nature as well as the intensity of the sounds were singularly modified by the molecular state of the wire submitted to the experiment. I had particularly mentioned the influence of temper and annealing, of greater or less tension, and of temperature. I had shown that iron wire, when under the influence of an action which renders it magnetic, does not emit the same sound as when it is in its natural state. Finally, by modifying, through the agency of heat, the molecular arrangement of some metals, such as platinum and brass, I had succeeded in obtaining from them, during the passage of the interrupted current, sounds, which, though feeble, were yet distinct.

The preceding reflections tended to confirm me in my opinion, that sounds produced under the influence of magnetism in the experiments on the voltaic arc, are owing to a molecular modification effected by the action of the magnet, and the more so inasmuch as the voltaic arc may be regarded as produced by a succession of interrupted currents, following each other with extreme rapidity, rather than by a perfectly continuous current. I accordingly took bars of other metals besides iron, as of tin, zinc, lead, bismuth, &c. I placed them on the poles of the electro-magnet, and I caused an interrupted current from a Grove's battery of from five to ten pairs to traverse them. They emitted no sound as long as the electro-magnet was not magnetized, but as soon as it was, sounds were very distinctly heard, composed of a series of strokes corresponding to the interruptions of the current, and analogous to that produced by a toothed wheel. The bars were 18 inches long, and from 9 to 10 lines square. Rods of copper, platinum, and silver produced a similar effect; a rod of iron did not emit a much louder sound under the influence of the magnet than it did when not exposed to this action.

What appeared to me most remarkable was, to find lead, a body so little elastic, yield a sound as powerful as those proceeding from the other metals, when placed under the same circumstances. The position of metallic bars with respect to the poles of the electro-magnet did not in any way modify the result of the experiment; they might be placed axially, that is to say, in the direction of the poles, or equatorially, that is, across the poles; the effect remained the same, being merely weakened as the distance between the bar and the poles increased. In order to hear the sound distinctly, when not very powerful, it was sufficient to establish a communication between the metallic bar and the ear by means of a wooden rod. In this manner the sound was not unfrequently heard prolonged some seconds, though growing constantly feebler, until it ceased entirely, after the source of magnetism had been withdrawn from the electro-magnet. Mr. Faraday has remarked an analogous fact in the action of the transparent medium on the polarized ray, an action which does not cease immediately with the magnetism of the electro-magnet. Is this prolongation owing to the magnetization of the electro-magnet not ceasing in a sudden manner; or to its return to its primitive molecular state not taking place instantaneously in the

substance submitted to its action? This question I am unable to decide. I incline, however, rather to the latter of these explanations, seeing that the effect is not equally perceptible in all bodies, and that it is, for example, more sensible in a bar of bismuth than in one of copper.

It is needless to remark that the calorific action of the current could not have any influence on the production of the phenomenon, since there could have been no development of heat, on account of the dimension of the bars compared with the force of the current. Besides, if the expansion arising from the heating of the body traversed by interrupted currents had caused the sound, the effect would have been produced equally, whether the bar had been under the influence of the magnet or not. This last remark applies equally to the following experiments, as to the preceding.

The intensity of the sound appears to depend much less on the nature of the substance submitted to the experiment, than on its form, its volume, and its mass. Tubes of platinum, of copper and of zinc, emitted more marked sounds than massive cylinders of the same metals. I wound a leaden wire in the form of a helix round a cylinder of wood; I did the same with a very fine platinum wire, and also with copper, zinc, and tin wires, taking care to place the coils of the helices so far apart that each should be isolated. Placed like bars and tubes, whether in the direction of, or across the poles of the electro-magnet, these helices emitted very powerful sounds when, the electro-magnet being charged, they were traversed by the interrupted current. It was particularly surprising to hear the lead wire emit so strong a sound. A helix constructed with copper wire, covered with silk, and composed of several coils wound round each other, emitted a very intense sound; it also emitted one, but much feebler, under the action of the electro-magnet.

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It is almost needless to remark, that in all these experiments an ordinary magnet produces the same effect as an electro-magnet. But what is more interesting, is to replace the action of the electro-magnet by that of a helix traversed by a strong continuous current, in the axis of which helix is placed the bar, the tube or the coiled wire, through which the interrupted current is transmitted. Experiments have shown me that in this case the results are the same; the intensity of the sounds is not very different, especially when tubes and wires coiled as helices are used.

If, between the exterior helix and the metal submitted to the action, a tube of soft iron is placed, the effect is a little heightened: it is neither increased nor lessened when the tube is of copper, only in this case another sound is heard which seems to proceed from the copper tube. This tube, however, is not traversed by a current, but it is probably acted upon by the currents of induction, which the interrupted currents traversing the conductor placed in the axis of the helix produce in it. I constructed a double helix formed of two thick copper wires covered with silk and coiled, each forming several circumvolutions, the one exterior to the other. In making a continuous current pass through the exterior wire, and an interrupted

current through the interior one, I heard a remarkably intense sound. In the reverse case, the sound existed, but was much weaker. This fact is evidently connected with the known property of helices traversed by electric currents exercising scarcely any magnetic influence exteriorly, whilst in the interior this action is very energetic.

Metals and solid bodies are not the only substances which produce the phenomena I have just described; all conducting bodies, whatever may be their state or their nature, appear to be capable of producing them. Thus, I have observed them with pieces of charcoal of all kinds and shape. Mercury also produces them in a very marked manner. I have inclosed mercury in a tube of glass an inch in diameter, and ten inches long: the tube was completely full and closed with care, so that the mercury could have no motion. As soon as it was traversed by an interrupted current, transmitted by means of two platinum wires, and the electro-magnet or the helix was made to act upon it, a sound was heard remarkable for its intensity. When the mercury was placed in an open trough, instead of being inclosed in a tube, it likewise produced a sound, and in addition there was seen on its surface an agitation or vibratory motion, very different from the gyratory motion observed by Davy, which appears under the influence of the poles of a magnet when traversed by a continuous current.

Dilute sulphuric acid, and what is even better, salt water, were successively put in a capsule of platinum placed on one of the poles of an electro-magnet. A point of platinum immersed in the liquid, served, together with the capsule, to send an interrupted current through it. A sound was again heard, but less distinct, on account of the noise produced by the disengagement of the gas: still it was so clear that no doubt could be entertained of its existence.

It may perhaps be thought that in the experiments I have just described, the sounds are produced by the mechanical action of attraction or repulsion exerted by the electro-magnet on the substance traversed by an interrupted current, and that, consequently, magnetism has no more share in the phenomenon than a finger might be supposed to have, when pressing on a sonorous cord. The simple description of the experiments shows this interpretation to be inadmissible. In the first place, the sound is the same with the wires in a helix, whether these wires be stretched or not, or whether they be of lead, platinum, or brass. Besides, how could this account for the sound produced in large masses, especially in liquids, such as mercury, and for the fact, that the position of the conductor traversed by the interrupted current with regard to the poles of the electro-magnet does not exert any influence on the phenomenon? Farther, it must be remarked that the sound in question is not a musical sound, such as would be produced by a string or mass made to vibrate by a cause acting exteriorly at its surface; it is a series of sounds corresponding exactly to the alternations of the passage of the current; like a species of collision of the particles amongst themselves. Thus, the phenomenon is molecular; and it leads to the demonstration of two important principles.

The first principle is, that the passage of the electric current modifies, even in solid bodies, the arrangement of the particles; a principle which I have already deduced from the experiments contained in my preceding memoir on this subject. The second principle is, that the action of magnetism, under whatever form it may be exerted, modifies alike the molecular constitution of all bodies, and that this modification lasts as long as the cause producing it endures, and only ceases with it. What is the nature of these two modifications? This is what we must endeavour to investigate and to ascertain. I purpose to engage in this inquiry, and indeed I have already made some attempts of which it would, however, be premature to give any account. I shall confine myself at present to a single remark, which does not appear to me to be devoid of interest: it is, that the influence of magnetism on all conducting bodies seems to impress on them, as long as it lasts, a molecular constitution similar to that which iron, and generally all bodies susceptible of magnetism possess naturally; for it developes in them the property of producing, when traversed by interrupted currents, sounds identical with those emitted also by iron and other magnetic bodies when transmitting these currents, but produced in these last without requiring the action of a magnet.

V. *On the Lunar Atmospheric Tide at St. Helena.*

By Lieut.-Colonel Edward Sabine, *R.A., For. Sec. R.S.*

Received December 16, 1846,—Read January 28, 1847.

The attractions of the sun and moon occasion tides in the atmosphere similar to those of the ocean. The effects are, however, so inconsiderable in comparison with the disturbances produced in the equilibrium of the atmosphere by other causes, that hitherto observation has not succeeded in affording any clear or decisive evidence of them. The phenomenon however has a very considerable philosophical interest as a farther exemplification of the universality of the principle of gravitation.

As the ebb and flow of the atmospheric tide must be greatest in the vicinity of the equator, and as moreover the ordinary equilibrium is less disturbed by irregular causes in that region of the globe than in the temperate zones, it was reasonable to expect that the existence of a tide in the atmosphere, depending on the position of the sun or moon relatively to the meridian of the place, would be shown, if at all, by observations made within the tropics; and that the most favourable situation for such observations might be that of a small island surrounded by a considerable extent of ocean, and therefore comparatively free from the atmospheric disturbances occasioned by variations of the terrestrial surface; and where also the tides of the ocean should be small.

The importance of St. Helena as a station of observation for this purpose early engaged the attention of the highly intelligent director of its magnetical and meteorological observatory, Captain Lefroy of the Royal Artillery, as may be seen from the following extract of a report received from that officer, dated June 1st, 1842.

"I believe that no observations have as yet decided the question, whether any effect upon the mean barometrical pressure is produced by the moon's daily passage of the meridian. The existence of an atmospheric tide of this nature appeared however so interesting a subject of inquiry, and its detection so probable, owing to the extreme uniformity of the daily oscillation, that the observations of seventeen months, viz. from August 1840 to December 1841 inclusive, have been arranged with that view. The mode adopted was this:—the corrected height of the barometer (*i.e.* the reading reduced to 32° Fahr.) at the hour of observation nearest the moon's meridian passage for every day has been entered in a central column; and in parallel columns headed -2^h, -4^h, &c. and $+2^h$, $+4^h$, &c. have been entered the observations taken respectively at 2^h, 4^h, &c. before and after the central observation. A mean has been taken for the observations included in each lunar month. It appears from the

seventeen months thus examined, that a maximum of pressure corresponds to the moon's passage over both the inferior and superior meridians, being slightly greater in the latter case; and that a minimum corresponds nearly to the rising and setting, or to $\pm 6^h$. The average of the seventeen months gives the respective pressures as follows, viz.—

	in.
Moon on the meridian	28·2714
Moon in the horizon	28·2675

The difference being ·0039 in.

"The latitude of the observatory is $-15° 57'$; the height above the sea, ascertained by levelling, 1764 feet to the cistern of the barometer. Observations made in 1827 under the direction of Major-General Walker, gave the particulars of the oceanic tides as follows:—

Rise at new moon . .	3 feet 6 in.
Rise at full moon . . .	2 feet 9½ in.
Rise at the quarters . .	1 foot 5 in.
Establishment	$2^h, 20^m$.

"There appears to be no establishment in the atmospheric tide, consequently the rise in the ocean will not account for the variation in the height of the barometer, because the times of maximum do not coincide."

Early in 1842 Captain Lefroy was succeeded in the charge of the observatory at St. Helena by Captain Smythe of the Royal Artillery; and from the 1st of October of that year, the observations, which up to that period had been taken at every second hour of mean solar time, were taken at every hour, and thus in their re-arrangement in lunar time, the mean height of the barometer at the different lunar hours became better represented than had been the case under the two-hourly system. During the first year of the hourly observations, viz. from October 1842 to September 1843 inclusive, the examination of the moon's influence continued to be carried on at the observatory by Captain Smythe in a somewhat modified manner, which is thus described by him:—"The hourly observations for each day, extracted from the day-book, were grouped into lunar months, and the monthly mean for each hour found. The observation taken each day at the hour nearest the time of the moon's meridian transit was inserted in the centre column for that day, and the other observations disposed right and left in order. As however the observations in this state are affected by the diurnal variation, which at St. Helena is very regular and considerable, and by which the moon's effect would have been overridden, the monthly mean for the hour was subtracted from each observation taken at that hour, and the remainder regarded as due to the moon's action. When only twenty-three observations intervened between two meridian transits of the moon, the middle observation was entered in both the -12 and $+12$ column."

Variation of the Barometer at the several Lunar Hours, from October 1842 to September 1843 inclusive, in decimals of an inch, + signifying an excess of barometric pressure, and − a defect.

1st. From the superior to the inferior passage.

Moon on the upper meridian.	+1h.	+2h.	+3h.	+4h.	+5h.	+6h.	+7h.	+8h.	+9h.	+10h.	+11h.	+12h.
+·0014	+·0011	+·0002	·0000	−·0008	−·0010	−·0012	−·0018	−·0011	−·0005	+·0002	+·0012	+·0012

2nd. From the inferior to the superior passage.

−12h.	−11h.	−10h.	−9h.	−8h.	−7h.	−6h.	−5h.	−4h.	−3h.	−2h.	−1h.	Moon on the upper meridian.
+·0016	+·0012	+·0009	+·0002	−·0006	−·0010	−·0011	−·0010	−·0004	+·0004	+·0003	+·0016	+·0014

We have here an average excess of barometric pressure of ·0014 in. at the hour when the moon is on the meridian above or below the pole, and an average defect of ·00115 at the period when she is six hours distant from the meridian; making together an average difference in the height of the barometer of ·00255 in. between the hours when the moon is on the meridian and when she is six hours distant from it.

The arrangement of the observations for the investigation proving rather a heavy charge on the establishment at St. Helena, the examination was now taken up at Woolwich, and carried through a subsequent period of two years, *i. e.* from October 1843 to September 1845 inclusive, for which the following method was adopted:—

If we call b the height of the barometer at 32° at any hour of observation, and $\overline{b}$ the mean height of the barometer at the same hour during the month to which the day belongs, then $b-\overline{b}$ is a quantity which remains over after the approximate diurnal variation has been eliminated; it is + when the barometer is higher and − when it is lower than the normal state, and expresses by how much the barometer is higher or lower at that observation than the normal state at the same hour. Monthly tables are then formed in which are inserted the values of $b-\overline{b}$ for each hour of mean solar time. The mean solar hours which are respectively nearest to the several lunar hours are then computed for every day, and the values of $b-\overline{b}$ are re-arranged in lunar monthly tables. The means at the several lunar hours in each month are then taken; and these means are finally arranged in periods of six months, yielding mean values of the barometrical variation at the several lunar hours for each half year. These are shown in the following tables:—

1st. From the superior to the inferior passage.

	Lunar hours.											
	0.	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.
Oct. 1843 to March 1844 ...	+·0017	+·0012	+·0004	−·0007	−·0021	−·0018	−·0018	−·0016	−·0019	−·0013	−·0004	+·0014
April 1844 to Sept. 1844 ...	+·0023	+·0020	+·0015	+·0007	−·0001	−·0013	−·0028	−·0027	−·0011	−·0004	−·0006	+·0009
Oct. 1844 to March 1845 ...	+·0021	+·0026	+·0023	+·0014	+·0007	+·0001	−·0007	−·0003	+·0005	+·0010	+·0017	+·0006
April 1845 to Sept. 1845 ...	+·0009	+·0001	−·0008	−·0018	−·0021	−·0024	−·0032	−·0024	−·0008	+·0008	+·0016	+·0026
Mean of the 1st year.....	+·0020	+·0016	+·0009	·0000	−·0011	−·0016	−·0023	−·0022	−·0015	−·0009	−·0005	+·0012
Mean of the 2nd year	+·0015	+·0014	+·0008	−·0002	−·0007	−·0012	−·0020	−·0014	−·0002	+·0019	+·0016	+·0016
Mean of the four half years	+·00175	+·00148	+·00085	−·00010	−·00090	−·00135	−·00212	−·00175	−·00082	+·00002	+·00058	+·00138

2nd. From the inferior to the superior passage.

	Lunar hours.											
	12.	13.	14.	15.	16.	17.	18.	19.	20.	21.	22.	23.
Oct. 1843 to March 1844 ...	+·0021	+·0020	+·0015	−·0006	−·0002	−·0005	−·0008	−·0007	+·0003	+·0012	+·0016	+·0018
April 1844 to Sept. 1844 ...	+·0005	−·0002	−·0007	−·0009	−·0015	−·0014	−·0015	−·0005	+·0003	+·0018	+·0024	+·0027
Oct. 1844 to March 1845 ...	+·0011	+·0007	−·0004	−·0024	−·0024	−·0026	−·0026	−·0023	−·0018	+·0001	·0000	+·0005
April 1845 to Sept. 1845 ...	+·0027	+·0022	+·0018	+·0010	−·0006	−·0015	−·0024	−·0023	−·0021	−·0001	+·0006	+·0012
Mean of the 1st year.....	+·0013	+·0009	+·0004	−·0008	−·0009	−·0010	−·0012	−·0006	+·0003	+·0015	+·0020	+·0023
Mean of the 2nd year	+·0019	+·0015	+·0007	−·0007	−·0015	−·0021	−·0025	−·0023	−·0020	·0000	+·0003	+·0009
Mean of the four half years	+·00160	+·00118	+·00055	−·00072	−·00118	−·00150	−·00182	−·00145	−·00082	−·00075	+·00115	+·00155

The evidence is here of the most decisive character of a barometrical maximum at the lunar hours of 0 and 12, and a minimum at 6 and 18, with a corresponding progression at the intermediate hours.

If we now arrange these results in such manner that the hours are combined in which the moon is similarly situated in respect to the meridian, we have the lunar horary variation of the barometer as follows :—

Moon's distance from the meridian.	Variations of barometric pressure.				Horary variation.			Moon's distance from the meridian.
	At the hours following the meridian passage.		At the hours preceding the meridian passage.		From the observations at the hours following the meridian passage.	From the observations at the hours preceding the meridian passage.	Mean.	
h	h in.	in.	h in.	in.	in.	in.	in.	h
0	0 +·00175 12 +·00160	+·00168	0 +·00175 12 +·00160	+·00168	+·00365	+·00365	+·00365	0
1	1 +·00148 13 +·00118	+·00133	11 +·00138 23 +·00155	+·00146	+·00330	+·00343	+·00336	1
2	2 +·00085 14 +·00055	+·00070	10 +·00058 22 +·00115	+·00086	+·00267	+·00283	+·00275	2
3	3 −·00010 15 −·00072	−·00041	9 +·00002 21 −·00075	−·00036	+·00156	+·00161	+·00158	3
4	4 −·00090 16 −·00118	−·00104	8 −·00058 20 −·00082	−·00070	+·00093	+·00127	+·00110	4
5	5 −·00135 17 −·00150	−·00142	7 −·00175 19 −·00145	−·00160	+·00055	+·00037	+·00046	5
6	6 −·00212 18 −·00182	−·00197	6 −·00212 18 −·00182	−·00197	00000	00000	00000	6

It should here be remarked, that as the observations were made at hours of solar time, they could not of course strictly correspond in the majority of cases to the lunar hours under which the value of $b-\overline{b}$ have been arranged. The effects of this imperfect synchronism have a tendency to compensate each other, and are probably nearly compensated, at all the lunar hours except at 0 and 12, 6 and 18. These, however, are the most important hours for the determination of the maximum effect, and, supposing there to be no establishment, it is obvious that whether the observation precedes, or whether it follows the precise lunar hour to which it should correspond, the error produced will be the same in kind, viz. the maximum at 0^h and 12^h will be lowered, and the minimum at 6^h and 18^h will be raised; and in both cases the error will tend to diminish the apparent influence of the moon's position on the barometric pressure. It may therefore be presumed that the true horary variation corresponding to the lunar hours is in every case greater than the numbers which appear in the preceding table; and that the horary variation at 0^h and 12^h which should express the whole difference of pressure corresponding to the moon's positions on the meridian and at six hours' distance from it, is especially less than the true amount, being diminished by the causes above mentioned, both at the hours when the moon is on the meridian and when she is six hours distant. To obviate this inconvenience and to give the results in future as much precision as they are capable of, eight additional observations, at equal lunar intervals, will hereafter be made in each day, corresponding precisely to the lunar hours of 0, 3, 6, 9 . . . . . 21; and these observations will be made on Sundays as well as on other days.

We may conclude therefore as the result of the two years of observation from October 1843 to September 1845 inclusive, that the barometer at the Observatory at St. Helena is higher when the moon is on the meridian, either above or below the pole, than when she is six hours distant from the meridian, by an average quantity which exceeds ·00365 in., and may be taken in round numbers as ·004 in.

For the purpose of examining whether any perceptible difference in the influence of the moon on the barometric pressure could be detected at the periods of the apogee and perigee, the following method was adopted. The epoch of the moon's perigee or apogee being taken from the Nautical Almanac, the nearest of the lunar hours 0, 6, 12, or 18 to that epoch is taken as the middle term of comparison; if it be 0 or 12 hours, the values of $b-\overline{b}$ at the four antecedent and four subsequent lunar meridian hours (0 and 12) are taken in addition to the middle term, to give a mean value corresponding to the times when the moon is on the meridian above or below the pole. The mean of the eight intermediate values of $b-\overline{b}$ at 6 and 18 hours taken in like manner, furnishes a mean value corresponding to the times when the moon is six hours distant from the meridian. The difference between these mean values of $b-\overline{b}$ gives for the epoch in question, the excess of the barometrical pressure when the moon is on the meridian above the pressure when she is six hours distant from it. Theoretically, this excess should be greater at periods of perigee than at those of apogee.

If the lunar hour nearest to the epoch of apogee or perigee be 6 or 18 instead of 0 or 12, that hour is taken as the middle term of comparison, and the mean values of $b-\overline{b}$ consist in such case of the mean of nine values at 6 and 18 hours, and of eight at 0 and 12. When a Sunday intervenes the same number of values of $b-\overline{b}$, viz. eight of the one and nine of the other, are taken to give the mean value in the comparison, which consequently in such case extends to a somewhat greater distance on either side of the middle term than when no such interruption occurs.

The mean excess of barometrical pressure when the moon is on the meridian, resulting from this comparison, is as follows :—

From 13 epochs of perigee between October 1843 and September 1844	·00407 in.
From 13 epochs of perigee between October 1844 and September 1845	·00394 in.
From 13 epochs of apogee between October 1843 and September 1844	·00341 in.
From 14 epochs of apogee between October 1844 and September 1845	·00347 in.

The number of observations from which each of the first three results is obtained is 221 ; in the fourth case it is 238.

We have here in both years a consistent indication of the greater influence of the moon on the barometrical pressure at periods of perigee than at those of apogee.

The effect of the solar action on the atmospheric pressure is far more difficult to be shown by any mode of grouping the observations than is that of the moon, not only because it is much smaller, but because it is masked in the diurnal barometric oscillation by the much greater and variable influence of the solar heat in producing atmospheric variations. That it is eliminated, in common with the other variations which depend on the sun's horary angle, by the process described in this paper, may be inferred from the fact that no significant difference appears in comparing the excess in the values of $b-\overline{b}$ at the lunar hours of 0 and 12, at the periods of syzygies and quadratures, during the twelve months from October 1843 to September 1844 inclusive. The mean values in this comparison have been taken from the observations at 0^h and 12, 6 and 18 for 36 hours preceding, and 36 hours following, the epoch of each syzygy and quadrature. The mean excess at 0 and 12 at the periods of syzygy is ·00337, and at those of quadrature ·00345.

The quantities treated of in this communication will justly appear to many persons as extremely small ; but the consistency of the partial results, when the observations are broken into periods of six months, places beyond doubt the power of observation, in appropriate circumstances and continued for a sufficient time, to determine both the existence and the approximate amount of a diurnal systematic affection of the barometer, not exceeding four thousandths of an inch. It also affords an instructive example of the beneficial influence of *mean numerical values* in the advancement of the physical sciences, and of the power which we possess through their means of progressively separating from apparently irregular phenomena, that which is constant and reducible to laws.

VI. *On the Diurnal Variation of the Magnetic Declination at St. Helena.*

By Lieut.-Colonel Edward Sabine, *R.A., For. Sec. R.S.*

Received January 21,—Read February 18, 1847.

It has long been known that in Europe the north end of a magnet suspended horizontally (meaning by the north end the end which is directed towards the north), moves to the *East* from the night until between 7 and 8 o'clock in the morning, when an opposite movement commences, and the north end of the magnet moves to the *West*. Recent observations have shown that a similar movement takes place at the same hours of local time in North America, and that it is general in the middle latitudes of the northern hemisphere.

It has also been known for some years past, and has been confirmed by recent observations, that in the middle latitudes of the southern hemisphere, the north end of the magnet moves in a contrary direction to that which has been described as taking place in the northern hemisphere, viz. that it moves to the west until 8 o'clock in the morning, or thereabouts, and then returns towards the east.

From the contrariety of the movement which is thus found to take place in the same meridians at the same hours in the opposite hemispheres, it seemed a not unreasonable conjecture, that at some intermediate point in each meridian, the causes, whatever they might be, which occasion these movements might counterbalance each other, and that the diurnal variation might consequently disappear: and questions were raised whether the line connecting these points in the different meridians would be found to coincide with the terrestrial equator, or with the line of no dip, or with one of the isodynamic lines.

The problem, which observation was thus called upon to resolve, has been stated with so much perspicuity by M. Arago in the Annuaire for 1836, that I may be permitted to reproduce it in his own words:—

"Dans l'hémisphère nord, la pointe d'une aiguille horizontale aimantée, qui se tourne vers le nord, marche, de l'est à l'ouest, depuis $8\frac{1}{4}^h$ du mat. jusqu'à $1\frac{1}{4}^h$ après midi; de l'ouest à l'est, depuis $1\frac{1}{4}^h$ après midi jusqu'au lendemain matin. Dans l'hémisphère sud, la pointe tournée vers le nord marche, de l'ouest à l'est, depuis $8\frac{1}{4}^h$ du matin jusqu'à $1\frac{1}{4}^h$ après midi; c'est précisement l'opposé du mouvement qu'effectue, aux mêmes heures, dans notre hémisphère, la même pointe nord.

"Supposons qu'un observateur partant de Paris s'avance vers l'équateur. Tant qu'il sera dans notre hémisphère, la pointe nord de son aiguille effectuera tous les matins un mouvement vers l'occident; dans l'hémisphère opposé, la pointe nord de cette

même aiguille éprouvera tous les matins un mouvement vers l'orient. Il est impossible que ce passage du mouvement occidental au mouvement oriental se fasse d'une manière brusque: il y a nécessairement entre la zone où s'observe le premier de ces mouvements, et celle où s'opère le second, une ligne où, le matin, l'aiguille ne marche ni à l'orient ni à l'occident, c'est-à-dire reste stationnaire.

"Une semblable ligne ne peut pas manquer d'exister; mais où la trouver? Est-elle l'équateur magnétique, l'équateur terrestre, ou bien quelque courbe d'égale intensité?"

In the recent work of the Baron von Humboldt (Cosmos, vol. i.), this question is also adverted to, and the problem is stated in nearly similar terms: after noticing the contrariety of movement in the two hemispheres, Baron von Humboldt remarks that attention has been justly called to the belief "that there must be a region of the earth, probably between the terrestrial and magnetical equators, in which no horary variation of the declination is sensible. This fourth curve, which might be called the curve of no motion, or rather *the line of no horary variation of the declination*, has not yet been found."

In the choice of the stations at which the magnetical observatories established by the British Government and by the East India Company in 1840 were placed, the solution of this problem was not overlooked. Singapore is situated close to the terrestrial equator; it was therefore well-suited to meet the suggestion of M. Arago in that respect: it is also not far distant from the line of no dip, and might be expected therefore to exhibit, in some degree at least, the peculiarities which might appertain to stations so circumstanced. In this country, a somewhat different mode of viewing the magnetic system of the globe from that which prevailed generally in France, caused an opinion to be entertained, that a different line from any of those suggested by M. Arago might not improbably prove the dividing line of the two magnetic hemispheres in this respect; and that the phenomena of the diurnal variation, whatever they may be, which should characterise the dividing line, might be most advantageously studied at a station chosen in its vicinity. The line here referred to passes round the globe, crossing the several meridian lines at points where the magnetic intensity in each is a minimum: or, it may be more precisely defined, as the locus of the points of minimum intensity of all lines on the surface of the earth drawn at right angles to itself. Its position has been traced through all the meridians of the globe with considerable approximation*. It is not by its definition necessarily an *isodynamic* line, or a line of equal magnetic force; and in fact, it is far from being so, the intensity of the force ranging in different parts of the line from 6·4 to 7·6 in absolute measure. It happens that Singapore, which, as already stated, is situated close to the terrestrial equator, and near the line of no dip, is in a part of the globe where the lines of least force and of no dip approach each other most nearly; consequently the observations at Singapore might be expected to exemplify the

* Reports of the British Association, 1837. Philosophical Magazine, vol. xiv. p. 81.

phenomena, whatever they might be, of an intermediate station, whether the intermediate character should be derived from proximity to the terrestrial equator, to the line of no dip, or to that of least force. St. Helena is situated close to the line of least magnetic force in a quarter of the globe where that line departs most widely both from the terrestrial equator and from the line of no dip; its latitude being about $-16°$ and the dip about $-22°$. Should therefore the diurnal variation at St. Helena be found to possess the intermediate character, it was considered that it would go far to indicate that the character was given by proximity to the line of least intensity, rather than to either of the two other lines. A third station, the Cape of Good Hope, seemed well-suited to subject this latter point to a still severer scrutiny; although somewhat more distant than St. Helena from the line of least intensity (which passes between those stations but nearer to St. Helena than to the Cape), the magnetic force at the Cape is still so weak as not to exceed in absolute measure the intensity on some parts of the line of least force (in the neighbourhood of Singapore for example): it was considered therefore as not improbable that if the intermediate character should prove to belong to stations at or near the line of least intensity, the Cape of Good Hope might be found to partake of the peculiarities of such a station, although the distance of the Cape from the terrestrial equator is not less than 34°, and the dip exceeds $-53°$.

From the moment when the observations of the first complete year at St. Helena and the Cape arrived in England and were examined, their bearing on the solution of the problem was perceived: but as opinions had been expressed of the probable influence of the cylindrical boxes in which the magnetometers were originally placed in generating currents of air at particular hours of the day and seasons of the year, and possibly of vitiating to a greater or less degree the diurnal variation thus observed, it was judged more prudent to suspend a notice of the inferences to which they led, until the observations of subsequent years made with additional precautions should have been received.

The results which will be now communicated to the Society, are founded on the observations at St. Helena of five years' continuance, viz. from 1841 to 1845 inclusive. From the beginning of 1841 to July 1843, the magnet was inclosed in a cylindrical box, corresponding to the description in page 14 of the Report of the Committee of Physics of the Royal Society; and from July 1843 to the end of 1845, in a double rectangular casing, which is thus described in a note from Captain Smythe of the Royal Artillery, Director of the St. Helena Observatory:—

"A double rectangular box was made to inclose the magnet and replace the cylindrical one with glass top hitherto used. It is composed of two boxes made of mahogany, every way similar to each other, the outer one being about an inch in all its dimensions larger than the inner one. The boxes are divided in the middle, and the halves fit close when pushed together by being half sunk into each other where they join. The interior of the inner box, and the exterior of the outer, are covered

with gilt paper, and the boxes are kept firm in their place by screws which pass through the cross piece connecting the copper pillars, and resting upon the top of the outer box."

On a careful comparison of the observations before and after this change was made, no material difference is perceived which can be ascribed to currents of air produced by either form of the magnetometer box. The law of the diurnal variation is the same whether it be deduced from the observations of the first period of $2\frac{1}{2}$ years, or from the second period of the same duration. The extent of the arc which the magnet passes through in the twenty-four hours as a consequence of the diurnal variation, whether it be measured by the difference of the extreme east and west positions of the magnet, or by the sum of the fluctuations observed from hour to hour, is not the same in different months of the year, and is also found to differ to a small amount in the same months in different years: but these are obviously real differences depending probably on occasional inequalities in their magnetic causes, and appear equally in either series considered separately. The law of the variation in each month may generally be derived from the observations of any one year of the series, but its average amount is more correctly derived from a mean of several years. I have therefore employed in this communication the mean of the five years, but without burthening the paper by inserting the details of the separate series.

The diurnal variation which we obtain from the observations of the five years at St. Helena shows that that station was well-chosen for the purpose of contributing to the solution of the problem in question: but the solution to which it conducts is of a very different character from that which was anticipated, and is one which seems not unlikely to assist materially in the eventual elucidation of the physical causes of the periodical variations. We have seen that in the northern portion of the globe the magnet moves to the east until seven or eight o'clock in the morning, and then returns to the west; it does so in every month of the year; the extent of the movement is greater in the summer than in the winter months, but the direction is always the same. So in the southern portion of the globe, the movement in the contrary direction is also constant throughout the year; it is greater when the sun is in the southern signs than when he is in the northern signs, but the direction is the same in all the months of the year; an extreme of westerly elongation is reached in every month about the hour of 8 A.M., as is an extreme of easterly elongation about the same hour in the opposite hemisphere. At St. Helena the well-marked peculiarity of the diurnal variation is, that during one-half of the year the movement of the north end of the magnet at the hours above referred to corresponds in direction with the movement which is taking place in the northern hemisphere, whilst in the other half of the year the direction corresponds with that which is taking place in the southern hemisphere. The opposite movements which take place simultaneously in every day of the year in the same meridians in the two hemispheres, do not by their mutual opposition neutralise each other and thus leave the magnet stationary. On the con-

trary, the diurnal variation at St. Helena partakes in, and possesses the characters of the phenomena of both hemispheres, but each predominates in its turn, prevailing separately and in opposite seasons. The passage from one order of the phenomena to the other takes place at or soon after the period of the equinoxes; in March and April, September and October, the diurnal variation at the hours referred to partakes, more or less on different days, of the characteristics of both seasons: but the months of May, June, July and August, on the one hand, and those of November, December, January and February, on the other, arrange themselves in wholly distinct categories; the north end of the magnet reaching its eastern extreme in the one case, and its western extreme in the other, nearly at the same hours; the extremes being moreover in both cases nearly equidistant from the mean position of the magnet in the respective months.

In Plate III. figs. 1 and 2 exhibit the projections of the diurnal variation at St. Helena from 3 A.M. to 3 P.M., in each month of the year, the projections representing the mean of five years of observations. In fig. 1, which contains all the months excepting March and April, September is seen to belong on the whole to May, June, July and August, although the influence of the opposite season is plainly visible at the hours of 18 and 19 (6 and 7 A.M.). October, on the other hand, must be classed with November, December, January and February, although an influence of opposite character is distinctly perceptible in the direction from 17^h to 18^h (5 to 6 A.M.)*. In fig. 2, the projections corresponding to the months when the sun has northern declination are collected into one darker line, as are those of the other five months when the sun has southern declination into another, for the purpose of exhibiting by comparison with the separate projections for March and April (which are the fainter lines), the degree to which the latter are intermediate. March and April have each both an eastern and a western elongation in the early morning hours; the eastern occurs one hour earlier than in May to September, and the western one hour later than in October to February: these peculiarities, both in regard to direction and to hours, are traceable without difficulty to the occasional alternation in those months of the influences of the opposite seasons.

Fig. 3. Plate IV. exhibits in the unbroken line the mean diurnal variation of the whole year at Toronto, and in the broken line the mean variation of the whole year at Hobarton in Van Diemen Island, showing the contrast which the *opposite hemispheres* present in this respect. In like manner the broken and unbroken lines in fig. 4 present in contrast with each other the diurnal variation of the *opposite seasons* at St. Helena. By the comparison of figs. 3 and 4, the general resemblance is shown

* The fact that October corresponds much more to November, December, January, and February than to May, June, July, and August is very decided; and is important to notice, because, although the sun passes to the south of the equator in September, he continues to the north of the parallel of St. Helena until the beginning of November. It is the sun's position in reference to the earth's equator, therefore, and not to the zenith of the place of observation, which marks the epoch of the change in the direction of the diurnal variation of the needle.

between the effect of the opposite hemispheres in the one case, and of the opposite seasons in the other, whilst at the same time the subordinate points of dissimilarity are equally conspicuous. It is not the object of this paper to enter on the discussion of minor points of difference, such as the non-agreement of the turning hours, which are somewhat earlier at St. Helena than at Toronto in the one case and at Hobarton in the other, and the check which appears to take place in the western elongation in May and September at St. Helena towards the hour of noon; but I may permit myself to notice, as a minor but apparently characteristic point of resemblance, the circumstance that the eastern elongation at the morning hour at Toronto, as well as in the corresponding season at St. Helena, always precedes by an hour the western elongation about the same period of the day at Hobarton and its corresponding season at St. Helena. This feature has a further tendency to connect a hemispherical peculiarity of daily occurrence throughout the year in each of the two hemispheres, with a periodical peculiarity at St. Helena conforming strictly to the alternation of the seasons.

The projections for Toronto and Hobarton in fig. 3 represent each a mean of two years of hourly observations; the scale on which they are drawn has been taken for convenience at half the magnitude of the scale on which the St. Helena projections in fig. 4 are drawn.

We have hitherto considered those portions of the diurnal variation at St. Helena which include the hours of the forenoon and of the first part of the afternoon. We have seen that from 3 A.M. to 3 P.M. the movement of the magnet is strikingly dissimilar, and is even opposite for a considerable portion of those hours, in the two solstitial periods of the year. A less marked but not less systematic difference takes place during the remaining hours, as is shown by the projections in fig. 4, which correspond respectively to the months from May to September, and from October to February. The diurnal variation at these hours, from May to September, consists in a small but continuous and steady motion of the north end of the magnet towards the east, commencing at 5 or 6 P.M., and continuing without interruption through the night until the following morning: whereas from October to February, the motion, which at first is in the same direction, is more considerable, and an inferior eastern extreme is reached about 9 or 10 P.M., to which there is nothing analogous at the other period of the year; a return then takes place towards the west (contrary to the direction in the opposite season), and is thenceforward continuous until the forenoon of the next day. In the night portion therefore of the diurnal curve as well as in that portion which has been more largely discussed, the horary variation at St. Helena does not disappear, but continues to exhibit a diversity at opposite seasons, in which an analogy may still be traced to the difference in the annual projections at Toronto and Hobarton at the same hours, shown in fig. 3. It should be noticed however that the correspondence which exists during the hours of the day between Toronto and those months at St. Helena which form the northern summer on the

one hand, and Hobarton and those months at St. Helena which form the southern summer, is not preserved in the night portion of the curve.

If the phenomena of the diurnal variation at St. Helena are characteristic of a station situated between the northern and southern magnetic hemispheres, partaking, although in opposite seasons, of those contrary features which separately prevail in the two hemispheres throughout the year, then we must regard the diurnal variation at the Cape of Good Hope, notwithstanding the remoteness of the situation of the Cape from the terrestrial equator and from the line of no dip, as supplying an additional illustration of the phenomena of an intermediate station. The projections in fig. 5, corresponding to the seasons May to September, and October to February, each representing also the mean of five years of hourly observation, bears a striking general resemblance to those of the preceding figure; the principal minor modifications also are such as may readily be imagined to be occasioned by the greater distance of the Cape from the dividing line. It will be remarked that the contrary movements at the opposite season of the year take place at the Cape of Good Hope as well as at St. Helena, although the sun is throughout the year to the north of the parallel of the Cape, and consequently is always north of its zenith.

Having named Singapore at the commencement of this paper, I may add that I have examined the manuscript observations of that station in 1841 and 1842, which are at the Royal Society: the observations appear to have been taken in those years at every second hour only, consequently the diurnal variation derivable from them is less complete than from an hourly record, but is quite sufficient to show that in general characters it corresponds with that of St. Helena and the Cape of Good Hope: the movement of the north end of the magnet in the months of May, June, July and August, is to the east until 7 or 8 A.M., followed by an immediate return to the west; and in November, December, January and February, to the west until 8 A.M., followed by an immediate return to the east: the last-named months have also an easterly extreme about 2 P.M., which has no similar or opposite feature in May, June, July and August.

From the facts here brought forward, it may be inferred that the line which has been supposed to exist by the eminent authorities referred to in the commencement of this paper, and which should be characterized by the absence of a diurnal variation of the declination, will not be found upon the globe.

VII. *On the Automatic Registration of Magnetometers, and other Meteorological Instruments, by Photography.* *By* CHARLES BROOKE, *M.B., F.R.C.S.E. Communicated by* G. B. AIRY, *Esq., F.R.S., Astronomer Royal.*

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AN efficient method of continuously registering the variations of magnetometers having been generally admitted to be a desideratum in science at the Magnetic Conference held during the Meeting of the British Association at Cambridge, it became a matter of philosophical interest to supply the deficiency, and this, it is hoped, has by the following means been satisfactorily accomplished.

As the mechanical force which a suspended bar magnet is capable of exerting, during its variations of direction, is far too minute to actuate the most delicate mechanism without sensibly affecting its position, the desired object will probably be obtained by photography alone. In order to render any method of photographic registration practically useful, it is essential that the three following indications should be fulfilled.

First, to obtain an easily managed artificial light of sufficient intensity to affect photographic paper, especially at those periods when it is of most consequence to obtain a continuous register, namely, when the position of the magnet is undergoing great and rapid variations.

Secondly, to prepare by a ready process photographic paper sufficiently sensitive to receive the feeble impressions of artificial light, and at the same time sufficiently durable to retain those impressions during a period of at least twelve hours, as a more frequent attention to the apparatus would probably interfere with the ordinary arrangements of an observatory.

Thirdly, to magnify the movements of the magnet by some optical arrangement, so that the variations may be indicated with sufficient minuteness and accuracy.

The first point may be attained by a camphine lamp, the light of which is much whiter and more intense than that of any other known lamp; that which has been in use (see fig. 3, Plate V.) is of a square form, each side being $2\frac{1}{2}$ inches long and $1\frac{1}{2}$ high, and stands in a little wooden frame made exactly to fit it, that it may with certainty be replaced in exactly the same position, if removed for the purpose of being trimmed. Many registers prove this to have been the case, there not having been any visible displacement of the line at the recorded period of removal of the lamp. The wick is flat, and about 0·5 inch wide, and inserted near one corner of the lamp, that the reflected pencil may not be obstructed in its passage; and a feeder

is inserted in the opposite corner, that the lamp may, if necessary, be supplied without removal; it will, however, contain more than enough for twelve hours' consumption. The plane of the wick is inclined at an angle of 4° or 5° to the axis of the pencil of light, which passes through a slit 0·25 inch long, and 0·01 inch wide in the side of a copper chimney: the chimney is supported on glass feet, to prevent the heating of the camphine by conduction, and for the same purpose the burner passes through a piece of wood. As the burner may occasionally require a vertical adjustment, it passes through a small collar with a set-screw; and as a minute quantity of resinous matter, resulting from the imperfect combustion of the camphine, will sometimes trickle down and interfere with the mobility of the collar, it is protected by a small concave rim attached to the burner. To prevent the escape of light from beneath the chimney, and also to protect the flame from the influence of slight currents of air, at the same time without excluding a due supply, a ring of metal is placed outside the glass supports, which rises about one-fourth of an inch above the bottom of the chimney. To render the combustion of the camphine more perfect, the draught in the chimney is confined by an obtuse conical diaphragm, having an aperture a little longer and wider than the burner, the exact size and position of which are represented in Plate V. figs. 4, 5, 6. The small obliquity of the wick is for the purpose of accumulating as much light as possible in a narrow pencil.

When the camphine is fresh, the lamp will burn for twelve hours without any deposition of carbon on the wick, or sensible diminution of the intensity of the light. The application of gas has not been attempted, as the apparatus would probably be applied in situations not accessible to the ordinary supplies of gas. For the purpose of actual observation, and especially wherever it may be proposed to obtain, not merely relative, but absolute determinations from the registers, it will be found desirable to detach the fine slit from the lamp, and to fix it by a suitable support to an immoveable stand on which the lamp is placed. It is also desirable that one edge of the slit should be adjustible by a fine screw, that the width of the aperture may be varied at pleasure*.

The second object has been attained by a paper prepared with the bromide of silver, which is well known to be affected by rays distributed over a larger portion of

* Since the paper was read the frequent use of the apparatus has suggested various modifications and improvements, both in its construction and practical application; these will be inserted in the form of notes for the sake of preserving the integrity of the original papers, and at the same time presenting all existing information on the subject.

The lamps now in use have been constructed by Watkins and Hill, having a detached slit with a fine adjustment (see fig. 7, Plate V.); much nicety is requisite in their arrangement, to enable the camphine to burn without smoking, at a point nearly approaching the maximum of illumination.

If the lamp and slit by which the base-line is described be similarly fixed in position, and the angular value of the distance between the base-line and the register at any given time determined, the declinations may be read off from the registers, with a limit of error depending on the length of scale and the definition of the line. —May 1847.

the spectrum than any of the other argentine salts, and is on that account preferable for the influence of artificial light. The sensibility of paper prepared with this salt has been found to be greatly augmented by the addition of a small quantity of iodide of silver (figs. 1 and 2, Plate V.). In fig. 1, the development of the impression has been arrested when distinctly visible on the iodized portions, but invisible on the remainder of the paper: in fig. 2, the difference in the clearness and darkness of the several portions of the line is very well marked. A little isinglass is also added, partly because the presence of a small quantity of organic matter appears to assist the catalytic action that ensues on the development of the impression, and partly for the purpose of retaining the salts dissolved with it on the surface of the paper.

The mode of preparation is thus: to a filtered solution of four grains of isinglass in one fluid ounce of boiling distilled water, add ten grains of bromide of potassium, and two grains of iodide of potassium*: when cold, the solution is evenly laid with a camel's hair brush on highly glazed paper in sufficient quantity to thoroughly wet the surface, but not to run off; the paper is then quickly dried by the fire, to prevent the solution being absorbed by the paper. Paper thus prepared may be kept for a considerable time in a dry place; but it is recommended not to prepare at one time more than enough for a week's consumption.

When about to be used, a piece of the above paper is evenly washed over by a camel's hair brush with a solution of fifty grains of crystallized nitrate of silver in one fluid-ounce of distilled water, by the aid of red or yellow light only, and placed damp in the apparatus, as the sensibility of the paper is much diminished by permitting it to become dry. When removed from the apparatus, the latent impression is developed by washing the paper with a saturated solution of gallic acid in distilled water, to which a very small quantity of strong acetic acid is added, when used; the addition of the acetic acid is found to diminish the darkening of the paper. The piece of paper used for each of these registers is half a sheet of folio post paper, torn lengthwise into two strips: to the quantity of gallic acid necessary to wet the surface of one of these pieces, which is about a teaspoonful, the addition of three drops of acetic acid is found to be sufficient. As soon as the impression is sufficiently developed, all soluble matter is removed by washing the paper two or three times in water; and lastly, the image is fixed by washing with a solution of twelve grains of hyposulphite of soda in one fluid-ounce of distilled water. A little practice in the manipulation is necessary for the success of this or any other photographic process. It may here be usefully remarked, that in all photographic processes, the strictest attention to cleanliness, as regards all instruments employed, is indispensably necessary: separate cloths, brushes and glasses should be used, and each retained for its respective purpose, as the smallest undue admixture of the materials will entirely frustrate the object to be attained.

The paper prepared as above described, is placed round the outside of a cylindrical

* See note, p. 67.

French shade, about 10 inches high and 14½ in circumference (figs. 1 and 5, Plate VI.), the corners being held together by a little gum dissolved in acetic acid. The shade, after having been blacked in the inside, is cemented into a cap 1 inch deep in the rim, and having a brass pin about 1½ inch long and a quarter of an inch thick fixed perpendicularly in its centre. A second shade, a little larger than the former, is then placed over it, leaving an annular space about one-eighth of an inch wide; the two cylinders are retained in a concentric position, by placing a sufficient number of turns of tape or ribbon round the rim of the cap of the inner cylinder, to fill up the intervening space. As the sensibility of the paper has been found to be materially diminished by allowing it to become dry, the tape or ribbon is saturated with water, previously to the outer cylinder being placed over it, and a piece of moist lint is placed between the round ends of the cylinders: the paper is thus kept perfectly damp until its removal, after having been in action twelve hours. The pin in the cap of the inner cylinder rests on a pair of friction rollers, and the cap rests against a horizontal roller, Plate V. fig. 8; the rounded end of the outer cylinder also rests upon a pair of rollers, which are adjustible in the direction of the common axis of the cylinders, Plate VI. fig. 1; the axis may thus be always rendered horizontal. By these means, the cylinders revolve so easily that they have been carried round by the hour-hand of a common watch; as however stoppages occasionally took place, from a deficiency of motive power, a stronger movement, such as now in use, is recommended*. A small collar with a set-screw fits on the pin of the cylinder, into which is inserted a bent wire that engages with a fork at the end of the hour-hand of the time-piece (Plate VI. fig. 3), the hour-hand being a little longer than the minute-hand. The cylinder is thus carried round its axis once in twelve hours, and at the commencement of the observation the carrier is so adjusted that the point of light may fall at the top of the paper. From the preceding description, it will be readily understood that by a combination of the vertical movement of the paper with the horizontal movement of the luminous point, the magnetic curve is traced out. The time-scale, which is measured lengthwise in the photographs, is evidently $\frac{1}{12}$th of the circumference of the inner cylinder to one hour, and the dimensions above mentioned, which allow 0·1 inch very nearly to five minutes, have been considered to define the period of any

* The size of the time-pieces in use is that of an ordinary ship chronometer, and in the construction of these a great improvement has recently suggested itself. The motion of the hour-hand is usually reduced from that of the arbor carrying the minute-hand by two wheels and pinions, the play of which, although necessary for their free action, allows a degree of mobility in the hour-hand rarely less than equivalent to five minutes in time, and consequently, without due caution, an error to this amount may be introduced in the register; moreover, the hour-hand is supported by too short an axis to enable it to overcome a considerable resistance applied to its extremity, without acting as a drag upon the whole train. To obviate these inconveniences, the hour-hand is placed on the arbor of the centre or second wheel, which is driven directly by the barrel, and drives the rest of the train. With this and other necessary modifications of the train, the variation of rate, whether the hour-hand moves freely, or is engaged in carrying the cylinder, will be scarcely sensible. In carrying out these views, the author is bound to acknowledge his obligation to the skill and intelligence of Mr. Elliott.—May 1847.

given variation with sufficient accuracy. As it is extremely improbable that two or more cylinders should be obtained of precisely similar circumference, it is desirable that a correct time-scale should be constructed for each cylinder employed, which may be readily effected in the following manner. Let a piece of paper, made as damp as the photographic paper usually is when placed on the cylinder, be cut so as to exactly meet round it; when dry, let this be divided into twelve equal parts, each of which will represent 1^h; these again into twelve parts, each of which will correspond to 5^m. By this arrangement an allowance is made for the shrinking of the paper*. The angular deviation is measured from a line on the paper drawn in the direction of its motion, such, for example, as would be described by the point of a fixed pencil resting constantly against it; and at the scale of 0·05 inch to one minute (which may perhaps be taken as the best working scale with the existing apparatus), the edge of the line when the various adjustments have been carefully attended to, is sufficiently well defined (as may be observed by a reference to many of the photographs) to determine its place to half or less than half a division of a scale of $\frac{1}{60}$th of an inch, that is, the position of the magnet may be read to within ten seconds. The plan adopted for determining the position of the base-line on the photographs, has been that of drawing pencil marks across the line at which one end of the paper overlaps the other, previously to its removal from the cylinder. When the register is dried, a line drawn through the two corresponding marks at the two ends has been taken to be the base-line. As however it can never be expected to obtain glass cylinders that have either perfectly cylindrical surfaces or perfect surfaces of revolution, the line of intersection of a plane perpendicular to the axis of the cylinder with the paper, would not probably be a perfectly straight line when the paper is opened out; and if the paper should from any cause contract unequally in drying, this line of intersection, which is the true base-line, would be liable to some further distortion. A comparison of many photographs with the Greenwich observations has led to the belief, that a small portion of error has occasionally arisen from these causes; there is, however, no reason for supposing that it has ever exceeded one minute. The means of obviating these sources of error by a photographic base-line are under consideration, but have not yet been perfected†.

* It is questionable whether the inequalities in length of the different registers, arising from different degrees of moisture and corresponding expansion of the paper, and from other causes, may not be too great to admit of the use of any fixed scale; in this case it will be necessary to divide each base-line into twelve parts by a suitable pair of proportional compasses, or some other contrivance. The errors arising from unequal expansion of different parts of the same piece of paper would, it is believed, be too minute to be recognised. In order, however, to guard against these sources of error, and generally for the purpose of marking any given epoch on the paper, the pencil of light by which the base-line is described is shut off by a sliding piece attached to the cover. After remaining so for a few minutes, it is readmitted for a period of ten seconds, and again shut off for a few minutes more, the time of its readmission being recorded. A small dot in the midst of an interrupted portion of the base-line will serve to determine the given epoch on the register, for an example of which see fig. 8, Plate IX.—May 1847.

† In the combined register of the declinometer and bifilar magnetometer, the base-line is now described by a

The following arrangements have been adopted to fulfil the third indication, that of magnifying sufficiently the movements of the magnet. A concave mirror of ten inches focus, and three inches aperture, is attached to a brass stem intervening between the frame which supports the magnet and the suspension chord. In order to reduce as much as possible the influence of torsion on the position of the magnet, the suspension skein consists of six equal bundles of untwisted silk fibres about six feet long, being portions of the same skein of flos silk; these, after being boiled in a solution of bichloride of mercury to render them less hygrometric, were stretched by six equal weights, the sum of which was very nearly that which the whole would subsequently have to support, and after having been left for three or four days to find their position of rest, were firmly bound together: by these means the probable equality of tension and freedom from torsion in the entire chord will, it is believed, be considerably augmented. The mirror may be moved round with the stem through any angle of azimuth, that the reflected pencil of light may proceed in a convenient direction; and the lamp must be placed at such a distance from the mirror that the conjugate focus may be at any required distance: and on this distance depends the amplitude of the scale, on which the variations of the magnet are represented. If the image of the slit is formed at a distance of 7 feet 2 inches from the line of suspension, each minute of angular motion of the magnet will be represented by a change of position of the line on the paper, amounting to $\frac{1}{10}$th of an inch; if formed at 9 feet $6\frac{1}{2}$ inches, 1′ will be equivalent to $\frac{1}{15}$th of an inch; if at 11 feet $11\frac{1}{4}$ inches, to $\frac{1}{12}$th of an inch; and if at 14 feet 4 inches, $\frac{1}{10}$th of an inch will represent 1′. The support of the lamp by which its adjustments are effected will be readily understood by an inspection of Plate I. fig. 1*.

The pencil of light forming the image of the slit is intercepted by a cylindrical lens, the axis of which is horizontal, and perpendicular to the vertical plane passing through the image and the centre of the mirror; the lens being placed at its focal distance from the image. By this arrangement, that portion of the pencil which passes through the lens is condensed vertically into a narrow space, without at all interfering with its horizontal movement. The most suitable focal length of the cylindrical lens depends upon the length of range: at the shortest distance abovementioned, a focal length of 1·5 inch will be found to answer very well; at the next, two inches; and at the longest, three inches.

As the amount of spherical aberration increases considerably as the eccentricity of the reflected pencil is increased, it is desirable that this eccentricity should be the least possible; with this view, the lamp must be so adjusted in the horizontal

separate lamp placed about 9 or 10 inches from the cylinder; a small pencil, the axis of which is perpendicular to the axis of the cylinder, is received by a lens placed at less than its focal distance from the paper, a small portion of the pencil thus condensed is transmitted through a narrow vertical fixed slit, and marks the paper (see fig. 8, Plate IX.). In the combined register of the balanced magnetometer and the barometer, the base-line is described by the barometer lamp, as mentioned in the Supplement.—May 1847.

* The lamp now stands on a fixed pillar to which the fine slit is attached.—May 1847.

plane, that the reflected pencil may pass close by the side of the chimney. It is obvious that the angle contained between the incident and reflected pencils may be diminished to any required extent by increasing the focal length of the mirror, as the lamp will then be placed at a greater distance from it, and by the same means the portion of light incident on the mirror is diminished, but the illumination of the image remains constant, its area alone being altered by varying the focal length of the mirror. Mirrors of 5, 6, 8½, 10, 12, and 15 inches focal length have been tried; and in conjunction with the cylindrical lenses which have been used, the mirrors of 8½ and 10 inches focal length and 3 inches aperture, appear to have produced the best effects. It may be here remarked, that the lenses used have been either cylinders, or portions of cylinders cemented together, filled with water; but with solid glass lenses, and more particularly with achromatic combinations, could such be obtained, it is probable that a better effect would be produced with mirrors of from 12 to 16 inches focus, and 3½ or 4 inches aperture*.

In the adjustment of the lamp, it is necessary that the plane of incidence and reflexion should be perpendicular to a vertical plane passing through the slit and the centre of the mirror. This adjustment may be most conveniently effected by placing a piece of wire horizontally across the slit and the reflected pencil, and having fixed the lamp at such an azimuth that the brightest portion of the pencil may be incident on the centre of the mirror, by moving the lamp vertically until the wire appears to coincide with its image at the centre of the mirror. By this adjustment, the image of the slit, being a portion of a caustic surface, is condensed in the direction of a tangent plane, and the confusion of the rays forming the luminous point is a minimum: the effect may be observed by comparing the line, fig. 3, Plate VII., in which this adjustment was intentionally disregarded, with fig 4, in which it was attended to, all other circumstances remaining precisely the same. It may be here observed, that the line being formed by a portion of a caustic surface condensed, the distinctness of its edge is independent of the distance at which the image is formed, and consequently of the

* The register, fig. 8, Plate IX., has been obtained by means of a mirror of 5 inches aperture, and 20 inches focal length; the reflected pencil being refracted through a combination of two plano-convex cylindrical lenses, one of which has a radius of 2 inches, and the other of 1 inch, the aperture of each being about 60 degrees, the interval between them about 1½ inch, and the more convex nearer to the paper. These lenses have been very beautifully executed by M. Lerebours. The mirror has been by successive trials carefully rendered elliptical, the conjugate foci being at the distances of about 2 feet, and 12 feet 4 inches from its surface. The latter distance was selected as suiting the locality of the Royal Observatory for which the apparatus was constructed. It may not be irrelevant to mention here the means by which the elliptical figure of the mirror was obtained. After being brought nearly to the proper figure, a screen was placed in front of it consisting of narrow concentric annuli of card, moveable on a wire, so that either of them could be placed perpendicular to the surface of the mirror, while the rest remained parallel to it, the lamp being placed at the distance of the nearer focus. By means of this screen the incident pencil might be received upon any annular portion of the mirror separately, and after many successive trials, the figure of the mirror was altered until an image was found to be formed by each annulus at the required distance. It is obvious that on the perfection of the figure of the mirror, the sharpness of the edge of the line will in a great measure depend.—May 1847.

amplitude of the scale. This may be observed by a comparison of figs. 1 to 20, Plate VII., in which the scale is 0·05 inch to 1′, with fig. 5, in which it is 0·1 inch to 1′: the only difference is in the darkness of the line, depending on the intensity of the point of light by which it is described.

In order to prevent the paper being darkened by the influence of stray light, a rectangular cover is placed over the cylinders, with a slit in the side, in the same horizontal plane as the axis of the cylinders, the slit being just wide enough to allow the point of light to pass through it: the paper is thus protected from the rays dispersed by the water lens. The time-piece, cylinders and lens, are placed on a tripod stand with the usual vertical adjustment (see fig. 1, Plate VI.), by which the whole apparatus may be so placed that the lens may receive the brightest part of the image. The surface of the stand, and all parts of the apparatus from which light could be reflected on to the paper are blackened over, and the whole is covered by a second case; in the side of which, towards the reflected pencil, is an aperture guarded by a tube about one foot long, and sufficiently large to admit the reflected pencil in any position that it may assume. The box in which the magnet is inclosed, to protect it from being disturbed by currents of air, and all other objects visible through the tube by an eye placed at the paper, except the mirror, are also rendered black; and so complete is this protection, that not the slightest difference can be perceived in the paper, whether bright daylight is freely admitted through three large windows, or wholly excluded. This apparatus has hitherto been applied to register the variations of the declination magnet only, but it may be considered equally applicable, with appropriate modifications, to record the variations of the horizontal and vertical elements of magnetic force.

The experiments have been conducted under the unfavourable influence of that constant tremor which exists in a London thoroughfare: this has been as far as possible counteracted by attaching to the magnet, about half-way between the point of suspension and its extremity, a piece of copper wire, the end of which dips into a glass vessel of oil; this does not appear to have interfered with the movements of the magnet*. The effect of the damper, in arresting the influence of local causes on the vibrations of the magnet, may be best appreciated by a reference to fig. 19, Plate VII., a register taken during a *maximum* local disturbance, namely, a quadrille party in the next house, from the party-wall of which the magnet has been suspended. The unsteady movement of the magnet during the period of the dance (the termination of which is very well-marked) is strikingly contrasted with those periodically augmented and diminished vibrations of the magnet about its mean place, the existence

* The use of oil as a damper is liable to the objection that it may congeal at low temperatures, and thus impede the movement of the magnet; and on this account a slip of wood attached to the magnet, and dipping into a glass vessel of mercury, has been substituted. The use of mercury seems moreover desirable on account of its gravity when so large a concave surface as that of a mirror of 5 inches aperture is exposed to the influence of currents of air, which would in all probability produce more or less vibration in the magnet.—May 1847.

of which has frequently been noticed, and which are very conspicuous in fig. 20, a portion of a register purposely taken during the most quiescent period of the week.

Notwithstanding the local sources of error above alluded to, it is very satisfactory to observe the close agreement between the photographs and several hundreds of the observations made at the Royal Observatory, which have been very carefully laid down upon them. These automatic registers include the two last term-days, and several periods of unusual disturbance, during which extraordinary observations have been made at Greenwich at intervals of one, two, or three minutes, and throughout the whole series the number of discrepancies is very small. An almost continuous registration has been maintained during the present year; and it is due to the praiseworthy vigilance of the observers engaged at the Royal Observatory to state, that very few extraordinary disturbances have escaped their notice.

Of those which have been compared with the Greenwich observations, fig. 20, Plate VII., the register of April 6th, 1846, and fig. 1, Plate VIII., that of April 16th, exhibit the greatest disturbance; in both instances it may be remarked that the greatest variation has occurred between 7^h and 11^h. It may here be conveniently remarked, that in accordance with the practice of the Royal Observatory, the time adopted is Göttingen mean astronomical time, which is $39^m\ 46^s$ in advance of Greenwich mean time.

Fig. 17, Plate VII., the register of April 15th, exhibits a brief disturbance, occurring between 8^h and 9^h, in which the photograph differs more essentially from the Greenwich observations than in any other instance: a sharp cusp in the latter, corresponding at two points to a mere bend in the former, would lead to the inference, that the disturbing cause must have been so near to Greenwich as to have exerted a materially greater influence on the magnet there under observation.

A singular fact is established by some of these photographs, namely, that after a certain space of time the actinic influence spontaneously decays; for in some instances the paper has been suffered to remain on the cylinder more than twelve hours; and consequently a second impression is made upon the paper within a very small distance of that which had been made twelve hours previously, and while the latter impression is distinctly developed, the former is very faint: fig. 6, Plate VII. is a good illustration of this fact. The succeeding portions however of the first impression become more and more distinct, thus showing that the decay is gradual. It appears to commence after a period of from ten to fourteen or fifteen hours; but the conditions on which this variation of time depends have not been ascertained*.

* Subsequent observation has led to the opinion that this decay of the impression depends on the paper having been originally prepared with too weak a solution of bromide of potassium. The solution now used contains 4 grains of isinglass, 16 grains of bromide of potassium, and 4 grains of iodide of potassium in one fluid-ounce of distilled water. The paper prepared with this solution appears to retain the actinic impression unimpaired for a period of more than 24 hours. Consequently two registers, each of 12 hours' variation, may be obtained on the same paper by allowing the cylinder to go twice round. An example of this may be seen in Plate IX. fig. 8, in which it may be observed that the occasional crossing of the lines does not at all interfere with their

The most violent shock that has been recorded, fig. 10, Plate VII., occurred between 5^h and 6^h on the 1st of April. On this occasion, the magnet, after suffering two small shocks, was at $5^h\ 20^m$ suddenly displaced as by a blow, and thrown into a wide oscillation, from which it did not return to a state of rest for 25^m; it then sustained a considerable shock in the opposite direction, from which it again returned to a state of rest, in nearly its original position, about 6^h.

It is confidently anticipated that a continuance of these observations during the approaching period of the year, when disturbances are usually most frequent and considerable, may lead to some interesting results.

distinctness. By this means half the trouble of changing the papers and developing the impressions is saved, and the relative positions at intervals of 12 hours may be determined with a much greater degree of certainty, the only source of error being in reading the position of the edge of the lines on a scale.—May 1847.

29 *Keppel Street*,
June 16, 1846.

VIII. *On the Automatic Registration of Magnetometers, and other Meteorological Instruments, by Photography. By* Charles Brooke, *M.B., F.R.C.S.E. Supplement. Communicated by* G. B. Airy, *Esq., F.R.S., Astronomer Royal.*

Received November 23,—Read November 26, 1846.

During the period of the summer recess, the system of automatic meteorological registration by photography has been rendered complete by the adaptation of the barometer and thermometer to the apparatus previously described. It having been found a matter of much difficulty to obtain a photographic base-line from the lamp already described as being placed near the magnet, the idea naturally arose that the base-line might be simultaneously described by a second lamp placed on the opposite side of the cylinder, as represented in fig. 1, Plate V. A pencil of light proceeding from this lamp through a horizontal slit in the chimney is received by a cylindrical lens placed, as before, horizontally, and the focal line of light thus formed is allowed to pass through a corresponding slit in the covering of the cylinder. A small section only of this focal line is transmitted through a vertical slit in a piece of thin sheet brass attached to the stand on which the cylinders rest, and placed very near the surface of the outer cylinder. A line thus described may be seen in Plate VIII. fig. 4, and Plate IX. figs. 6, 7, 8, and the same light has been by the following means rendered available for the registration of the barometer. A siphon barometer has been constructed with a column of mercury a little more than one inch in diameter, Plate VI. figs. 1 and 2. As the weight of an entire column of this size would be inconvenient, and as it would be difficult to obtain a tube more than three feet long of so large a bore, both ends of which were of the same internal area, two adjacent short pieces of a very nearly cylindrical tube have been united to the extremities of a tube of small bore, and form the ends of the instrument which contain the surfaces of the mercury. A wooden cap about two inches high is fitted to the open end of the tube, at each end of which are fixed three small friction rollers, placed radially, vertical, and equidistant from each other. The stem of a glass float, having a bulb about half an inch in diameter, resting on the surface of the mercury, passes up vertically between these friction rollers, by which the free vertical movement of the float is much facilitated. At the upper end of the stem is a cap containing a small grooved roller. The barometer tube is attached to a board by two clamps, so as to be capable of being raised or lowered at pleasure, and the bend at the lowest part rests on a piece of wood, which is likewise capable of a vertical adjustment. Another piece of wood, about half an inch thick, two inches wide, and five or six long, is made to slide horizontally between

two slips fixed to the surface of the board at such a height that the top of the float may be opposite the middle point between them. To this sliding piece a pulley about three inches in diameter, having a fixed axis about 3 inches long, is attached by a suitable support; to this pulley two slender wooden arms are attached, one thirty inches, the other five inches long, and fixed at right angles to each other*. A piece of wire with an adjustible balancing weight is fixed in the pulley in such a position that the axis of the pulley may be the centre of gravity of its appendages. The long arm passes through a slit in the stand of the apparatus, and carries a black paper screen with a vertical slit in front of the horizontal aperture in the cover above described (see Plate V. fig. 2); and is so placed that the point at which the slits cross each other is exactly thirty inches from the axis of the pulley. The short arm rests on the roller at the end of the float, and is marked at the distances of 3, 3·75, and 5 inches from the axis of the pulley. The mark which rests on the float may be changed at pleasure by sliding horizontally the piece to which the pulley is attached; and accordingly as the marks are respectively placed in the above position, it is evident that the movement of the point of light transmitted through the slit in the moveable screen will be five, four, or three times the variation in the height of the column of mercury; and thus by the same lamp the base-line and the barometric curve are traced out. Of this, fig. 4, Plate VIII. and fig. 7, Plate IX. are given as examples. In these it may be remarked that both the lines are so sharply defined, that by applying a scale divided into $\frac{1}{100}$ths of an inch, the position of both may be read to half a division, which is equivalent to 0·001 inch of mercury, if the first scale be adopted, which has been the case in these instances.

A small weight suspended by a string passing round a groove in the pulley keeps the short arm in contact with the float, by a constant pressure. There being an annulus of mercury rather more than one-fourth of an inch wide between the tube and the float, the effect of capillarity is so much reduced as to exert scarcely any influence on the variations of the column, the weight of which is sufficient to overcome the small amount of friction that exists in the various parts, without sensibly influencing its variation, and consequently the barometric curve is frequently continuous, and not interrupted by jerks. In one of the registers, not introduced for want of space, the passage of an aërial wave is recorded, equivalent to less than $\frac{1}{300}$th of an inch of mercury, the duration of which was about $4\frac{1}{2}$ minutes.

The lamp being placed at a distance of about nine inches from the paper, the direction of the small pencil by which the curve is traced varies considerably; hence an error is introduced in the register, equal to the distance of the slit from the paper multiplied by the cotangent of the angle at which the ray is inclined to it; this however may be either allowed for, or obviated by rendering all the rays of the pencil parallel to the same vertical plane by means of a cylindrical lens, placed at its focal distance from the lamp.

* The pulley and slide have since been made of brass.—May 1847.

A continuous registration of the variations of the thermometer has been obtained by intercepting the focal line of light formed on the paper as above described, by the stem of a thermometer having a wide flat bore. A sufficient quantity of light passes through the empty portion of the bore to darken the paper, but is entirely excluded from the portion occupied by the mercury. The register therefore consists of a light and a dark space, separated by a well-defined boundary line, the distance of which from the base-line will furnish the required indication. This particular application of the apparatus prefers no claim to novelty, as a very similar means of registering the variations of the thermometer has already been published*, and is here introduced merely as forming a necessary part of a complete system of automatic meteorological registration.

As a thermometer with a large bore, and a scale sufficiently open to give the indications of change with the requisite degree of minuteness, must of necessity contain a large quantity of mercury, which if contained in a globular bulb would not be sufficiently sensitive, the instrument which has been used has a long narrow tubular bulb (see Plate VI. fig. 11), by lengthening which any required amplitude of scale may be obtained without any diminution of sensibility.

As small differences of temperature have a much greater influence at low temperatures in determining the hygrometric conditions of the atmosphere, from a comparison of the thermometer and psychrometer, than they have at higher temperatures, and as the range of variation is so much less during winter than in summer, it is proposed that a thermometer and psychrometer, having scales of about five degrees to one inch, should be used in winter; while those for summer use should have a scale of about ten degrees to one inch.

It having been found practically impossible to depend on the uniformity of a wide flat bore in a glass tube, a more than usually correct method of graduating these instruments would be desirable. This object would be attained by fixing to the stem a scale of $\frac{1}{30}$ths of an inch, which by a suitable vernier might be read with any required degree of minuteness. A separate comparison of the readings of this scale with two or more good standard thermometers should now be made, each being immersed with the instrument to be graduated in a vessel of warm water, which is allowed to cool very slowly. A mean of the results thus obtained would probably afford a very nearly correct graduation, which would, for the reasons above stated, be of most importance at low temperatures.

If the register is required to furnish only differential results, a great length of stem may be obviated, and the safety of the instrument secured, if casually exposed to a temperature above its range, by a safety bulb at the upper end of the stem, as represented in the diagram. By retaining a certain quantity of mercury in this bulb, the mean temperature corresponding to the time of year may be always made to occupy nearly the same place, and a change of thermometers thus rendered unnecessary.

* See Engineers' Magazine, Nov. 1845.

The six meteorological instruments of which a continuous registration is proposed to be obtained by the means above described, are the declination magnet, the horizontal and vertical force magnetometers, the barometer, the thermometer, and the psychrometer; and by the arrangements above described, two of these may be registered on the opposite sides of each of three cylinders; the declination and horizontal force magnetometers on a cylinder placed horizontally, as represented in the Plate. In this case the describing a base-line would be most readily effected by a third lamp; The horizontal force magnetometer and the barometer on a vertical cylinder, the long arm of the barometer index being placed horizontally; and the thermometer and psychrometer on a second vertical cylinder, which must necessarily be placed in the open air. This arrangement will not however be attended with any difficulty, if the lamps are inclosed in a case similar to a magic lanthorn, the chimney of which is protected from the influence of descending currents, either by a revolving cap with a spiral lamina attached to it, or by FARADAY's ingenious expedient of interrupting the continuity of the chimney at two or more points, by parallel conical surfaces. The apparatus for carrying the vertical cylinders is represented in Plate VI. figs. 4 to 8.

Figs. 9 and 10, Plate VI., are views of a carrier for the bifilar magnetometer. The stem to which the mirror is attached has a stirrup to hold the bar at the lower, and a torsion circle at the upper end. To the index-plate which moves with stiff friction on the torsion circle is attached a right- and left-handed screw, carrying two pulleys under which the suspension skein passes. These may be adjusted by the screw to give the requisite degree of sensibility to the magnet. A more accurate adjustment of the angle of torsion may, if requisite, be obtained by a tangent screw attached to the index, and gearing with the circumference of the torsion circle.

Keppel Street,
November 23*rd*, 1846.

Postscript.—In the endeavour to avoid prolixity, the author may perhaps have omitted some details which would have facilitated the construction of the apparatus. On some few points, on which circumstances have appeared to render further information necessary, that has been embodied in the description of the Plates: he will, however, be at all times ready to further the objects of science, by communicating any required details to those who may be disposed to make a practical application of them.

The author takes this opportunity of publicly and gratefully expressing his thanks to the Council of the Royal Society for their liberal contribution towards defraying the expenses of the apparatus.—*May* 24, 1847.

Description of the Plates.

PLATE V.

Fig. 1. A sketch of the complete apparatus, as used in combining the registers of the declinometer and barometer. The combination of two plano-cylindrical glass lenses, as used in obtaining the register fig. 8, Plate IX., is here represented.

Fig. 2. A sketch of the barometer side of the apparatus, with lamp, &c. The cylindrical water lens, as originally used, is represented in this figure.

Fig. 3. A sketch of the lamp in its stand, to which the fine slit is attached.

Fig. 4. A section of the burner, the wooden socket, through which it passes, the chimney and the diaphragm; one-half the real size. The top of the burner should stand from 0·22 to 0·25 inch below the aperture in the diaphragm, and the wick should rise from 0·04 to 0·05 inch above the top of the burner. The illuminating power, and steady burning of the lamp without smoking, entirely depend on the correct adjustment of these distances, if the camphine is not oxidized by too long keeping.

Fig. 5. A sketch of the diaphragm, full size, showing a notch or depression of a portion of the edge intervening between the flame and the slit, for the purpose of allowing the passage of rays from all the most luminous portion of the flame, some part of which is below the level of the edge of the diaphragm.

Fig. 6. A plan and elevation of the diaphragm, full size; to show the size and position of the aperture.

Fig. 7. An elevation of the adjustible slit. As this is in contact with the chimney, its support necessarily becomes very hot; and in order to diminish the transmission of heat to the lamp, a space of about 0·1 inch is left between the support and the ring of metal described as surrounding the burner; the wings represented in this figure intercept the light that passes through the space here mentioned.

Fig. 8. The friction wheels which support the axis of the horizontal cylinders.

PLATE VI.

Fig. 1. A side view of the cylinders and time-piece, showing their connexion, and an elevation of the barometer, showing the several parts described. If the registers of the balanced magnetometer and barometer are combined as proposed, the relative positions of the index, its slit, and the cylinders will remain the same.

Fig. 2. A side view of the barometer.

Fig. 3. An elevation of the dial and hands of the time-piece, showing the fork at the

extremity of the hour-hand, that engages with the carrier on the axis of the cylinder.

Fig. 4. A sketch of the apparatus for carrying a vertical cylinder, with the requisite arrangement of lenses for combining the registers of the balanced magnetometer and barometer, or of the thermometer and psychrometer.

Fig. 5. A vertical cylinder, with its outer cylinder. A hole in the centre of the cap fits over a pin in the centre of the plate on which it rests, and is carried round.

Fig. 6. The horizontal plate on which the cylinder rests. The under surface of this plate rests on the edges of three equidistant vertical friction wheels or rollers, attached to the frame beneath it. A vertical pin fixed to the centre of the under surface of the plate passes down between the edges of three equidistant horizontal rollers, which are attached to the under surface of the frame, and on this pin the carrier is fixed.

Fig. 7. A frame supported on three legs, to which the friction rollers above described are attached. By this arrangement the motion is so easy that the effect of the drag on the rate of the time-piece is scarcely sensible.

Fig. 8. The time-piece, the rim of which is let into the surface of the stand. In doing this great care should be taken that the axis of motion of the hour-hand is continuous with the axis of the cylinders, and of the plate on which they rest. In registering the balanced magnetometer, as in the declinometer, the cylinder makes two revolutions in 24^h, and two 12^h lines are traced on the paper; but for the thermometers, the cylinder, and consequently the hour-hand makes only half a revolution in 24^h, and each of the two thermometers is registered on one-half the paper during that time. The cylinder is about 19 inches in circumference, which suits the length of a sheet of folio post writing-paper.

Fig. 9. An elevation of the bifilar carrier.

Fig. 10. A side view of the same. The centres of gravity of the carrier, the mirror, and the magnet, are very nearly in the same vertical line.

Fig. 11. An elevation of the thermometer tube.

PLATE VII.

Figs. 1, 2. Show the increased sensibility of the paper by the addition of a very small quantity of the iodide of potassium to the first solution.

Fig. 3. Shows the effect produced upon the line by the want of horizontal adjustment of the plane of incidence and reflexion, compared with

Fig. 4, in which this adjustment has been carefully made, all other circumstances remaining the same.

Fig. 5. Specimen of a well-defined line, on the scale of 10′ to 1 inch.

Fig. 6. The development of a recent impression, and the decay of another made twelve hours previously, and very near the former.

Figs. 7, 8. The comparative effects of the camphine and oil lamps, they having been exchanged for the purpose of experiment.

Fig. 9. The commencement of the magnetic storm of September 5th, 1846. The oil lamp was in use, and this shows its inability to impress the photographic paper during rapid movements of the magnet, when the registration is most important.

Fig. 10. The greatest magnetic shock that has been observed during the year*. It occurred on April $1^d\ 5^h\ 20^m$.

Figs. 11, 12, 14. Examples of shocks, of these No. 12 is the least impulsive. It may be remarked, that in these and in all other recorded shocks, the first impulse has been to the west, and has been followed shortly after by a second to the east.

Fig. 13. Tremor continuing for more than an hour. This occurred on April 25^d, between 20^h and 22^h, and from the habitual quietude of the streets at such a time, cannot probably be attributed to local causes.

Fig. 15. A register of July $12^d\ 3^h$ to 8^h, showing an unusually frequent repetition of small oscillations. These oscillations, which have been frequently noticed, differ considerably from the shocks; the magnet appears to be disturbed from a state of rest by gradually increasing oscillations, which, after having attained a maximum, subside in nearly an equal time: the period of these disturbances is from two or three to ten or fifteen minutes.

Fig. 16. The effect of a maximum local disturbance—a quadrille party in the adjoining house. The unsteady movement of the magnet is well-contrasted with the preceding disturbances.

Fig. 17. A disturbance which occurred on April $15^d\ 8^h$ to 9^h. This differs more essentially from the Greenwich observations than any that has been compared: in the curve laid down by points from the observations made at the Royal Observatory, two sharp cusps correspond in time to mere undulations in the register. May it not be inferred that in this instance the disturbing cause was sensibly nearer to Greenwich than to Keppel Street?

Fig. 18. The register of August $1^d\ 2^h$ to 11^h, during which period the great hail-storm occurred.

Fig. 19. A register obtained at the Royal Observatory, without a damper. The oscillations of the magnet gradually diminished, and after $6^h\ 40^m$ it returned to a state of rest.

Fig. 20. The register of April $6^d\ 1^h$ to 13^h, one of the great disturbances of the year 1846. A very close agreement may be observed between the register and the Greenwich extraordinary observations, made between 8^h and 10^h. In all the figures of this Plate, except fig. 5, the scale is that of 20′ to 1 inch.

* All the dates except that of fig. 8, Plate IX. refer to the year 1846.

PLATE VIII.

Fig. 1. Register of April $15^d\ 4^h$ to 12^h, another great disturbance. The Greenwich observations made during the principal part of the disturbance agree well with the register. Scale 20′ to 1 inch.

Figs. 2, 3. The greater part of the term-day, August $28^d\ 10^h$ to $29^d\ 10^h$, on the scale of 10′ to 1 inch. These registers show a general agreement with the Greenwich observations; but it appears that the excursions of the registered magnet have been generally less than those of the Greenwich magnet. No. 3 shows a constant succession of small disturbances.

Fig. 4. A register of the thermometer and barometer combined, the two instruments being placed on opposite sides of the cylinder. The base-line drawn by the barometer lamp may be distinguished through the paler tint produced by the light passing through the empty portion of the thermometer tube.

PLATE IX.

Fig. 1. Register of August 20^d, showing an unusual absence of disturbance: this is not however a solitary instance.

Fig. 2. Register of September $5^d\ 0^h$ to $11^h\ 30^m$, the greatest magnetic storm of the year 1846. It may be remarked, that the excursions of the registered magnet, though agreeing in direction, were considerably greater in extent than those of the Greenwich magnet. The contrary took place in figs. 2, 3, Plate III. This and the preceding are on the scale of 20′ to 1 inch.

Fig. 3. Portion of a register on another piece of the same sheet of paper as the preceding, but on the scale of 10′ to 1 inch; to show, *cæteris paribus*, the relative intensity of the actinic rays.

Fig. 4. Register of September $5^d\ 12^h$ to 23^h, showing the gradual subsidence of the storm. Scale 20′ to 1 inch.

Fig. 5. Portion of the succeeding register of September $6^d\ 0^h$ to 11^h, on part of the same sheet as 4. Scale 10′ to 1 inch. The line is equally well-defined, but fainter.

Fig. 6. Register of a term-day, September $23^d\ 10^h$ to 22^h, with a photographic base-line; on which the Greenwich observations were laid down at the Royal Observatory. This register well illustrates a fact that has been frequently noticed, namely, the gradual subsidence of slow excursions into small and brief disturbances. The scale is 15′ to 1 inch.

Fig. 7. Another of the great disturbances of 1846; November $17^d\ 0^h$ to 12^h with base-line, and register of barometric variations magnified five times. The nearly horizontal portion of the line between $6^h\ 20^m$, and 30^m, indicates the most rapid movement of the magnet that has been recorded by photography. No point of it could have been illuminated more than 16^s. The

scale is 15′ to 1 inch. This register exhibits a very remarkable accordance with the extraordinary observations made at the Royal Observatory during the latter part of the disturbance. Two sharp cusps at $8^h\ 2^m\ 30^s$ and at $8^h\ 25^m$ may be specially noticed. The advantage of automatic registration is here manifest; the magnet having been during several previous bihoral observations very steady, as may be seen from the first part of the line, there was no previous indication of the disturbance which commenced shortly after 6^h: and the attention of the assistant on duty was directed to the magnets only by his casually observing that the balanced magnetometer had undergone a very remarkable deflection. The sudden breaks in the barometer line are due to a small amount of friction that has since been obviated.

Fig. 8. A register of the declinometer from May 1847, $1^d\ 23^h\ 34^m$ to $3^d\ 0^h\ 4^m$, made in Keppel Street by the apparatus constructed for the Royal Observatory. There is no perceptible difference in intensity between the commencement and termination of the line. Several shocks may be remarked, the W. declination having been, as usual, increased by the disturbing cause, which appears to have commenced and terminated abruptly. In some instances the duration of very transient disturbances may be reasonably conjectured from the tint of the marks on the paper, those for instance at *a*, *b* have not probably exceeded 20^s. The absence of vibrations, as in the shocks represented in Plate VII., is probably owing to the inertia of the mercury damper, and offers an additional argument in favour of its use. The base-line is drawn by a lamp on the opposite side of the cylinder, and shows two of the 10^s marks previously described. The narrowness and sharpness of the base-line will materially assist in diminishing the probable error of reading the position of the register by a scale.

IX. *On the Proper Motion of the Solar System.*

By Thomas Galloway, *Esq., M.A., F.R.S., Sec. R.A.S.*

Received March 4,—Read April 15, 1847.

THE third volume of the Mémoires presentés par divers Savans of the Imperial Academy of St. Petersburg, published in 1837, contains a paper by Professor Argelander, in which that distinguished astronomer has discussed the question of the proper motion of the solar system, and determined the probable situation in space of the point towards which the sun is at present advancing. This determination was founded on the proper motions of 390 stars situated between the north pole and the tropic of Capricorn, as shown by a comparison of their positions in 1775 according to Bradley's observations, reduced by Bessel, with their positions in 1830 computed from the observations made by Argelander himself at Abo; every star being taken into account which appeared to have a proper motion amounting to a tenth of a second in space annually. Two other investigations of the same question have since been published; one by Lundahl, founded on the proper motions of 147 stars, as shown by a comparison of the observations of Bradley and Pond, and the other by Otto Struve, based on 392 stars, whose proper motions were determined by a comparison of Bradley's observations with those made at the observatory of Dorpat. From these three investigations the direction of the sun's motion in space may be considered, perhaps, to have been determined with as great an approximation to accuracy as can be attained in the present state of our knowledge of the proper motions of the stars in the northern hemisphere. The recent catalogues of Mr. Johnson and the late Professor Henderson, deduced from the observations made by those astronomers respectively at St. Helena and the Cape of Good Hope, on being compared with the Cape observations of Lacaille made about the middle of the last century, show that a considerable number of the southern stars have also very appreciable proper motions; and it appeared to me to be a matter of some interest to inquire whether the proper motions so determined afford any confirmation of the results obtained by Argelander, Lundahl and Struve, or favoured the hypothesis of a displacement of the solar system. The result of this inquiry I have now the honour of submitting to the Royal Society, in whose Transactions the existence of relative displacements among the fixed stars was first announced, and the probable direction of the sun's motion first indicated. Independently of theoretical considerations, the subject is of much importance in astronomy. The proper motions of the stars, which may be said to be the only residual astronomical phenomena now remaining to be

accounted for by theory, mix themselves up with the determination of the precession and other fundamental elements; and the first step towards acquiring any knowledge of their laws, quantities, or directions, is obviously to distinguish between what is real and what is only apparent, and to separate from the whole observed displacement the effect due to the motion of our own system.

Before proceeding to describe the data and results of the present investigation, it will be desirable, perhaps, to give a brief notice of the principal inquiries that have heretofore been undertaken with reference to the same subject.

In the Philosophical Transactions for 1713, Halley first called attention to the circumstance that a comparison of the ancient with modern observations showed that three of the principal stars, Sirius, α Tauri and Aldebaran, had changed their positions relatively to the fixed circles of the sphere, and advanced considerably towards the south. Until this time the notion had universally prevailed that the places of the stars are subject to no relative change; but no sooner was the notion called in question than instances of such change were multiplied; and the proper motions of stars being once admitted, it was naturally suggested that the sun itself partakes of a similar motion. Bradley, in the memorable paper in which he announced the discovery of the nutation, published in the Philosophical Transactions for 1748, described the appearances which would result from a change of the position of the solar system in absolute space, but he made no attempt to explain the observed phenomena on this hypothesis, and remarked that the alterations in the relative positions of the stars might arise from so great a variety of causes, that many centuries, perhaps, would be required to discover their laws.

Tobias Mayer, in a memoir presented to the Göttingen Society in 1760, and published among his Opera Inedita in 1775, gave a list of eighty stars which had been observed by Rœmer in 1706, and compared their places as given by Rœmer with those deduced from his own observations (1756) and those of Lacaille (1750). Out of the eighty stars about fifteen or twenty were found in respect of which the difference of position, either in right ascension or declination, exceeded $15''$, a quantity which he considered would be at least equal to the error of observation. In the cases, therefore, in which the difference did not much exceed $15''$, he thought a proper motion was not improbable; but in some cases, as those of Arcturus, Sirius, Procyon, α Aquilæ, Piscis Austrinus, and a few others, the difference was so great that there could be no question about the existence of such motion. Mayer also made the remark, that the stars which appear to change their places are not confined to those of the first or second order of magnitude, which, by reason of their greater brilliancy, might be presumed to be the nearest to the earth; and that among the brighter there are some which appear to be altogether at rest. And he further remarked, that although it is by no means improbable that the sun as well as the stars may have a motion of its own, yet, as the observed changes of position do not follow the law they would observe if caused by the motion of our solar system towards a given point in

the heavens, it is manifest they do not proceed from this or any similar common cause, but belong to the stars themselves, though the true and genuine cause may remain unknown for ages.

This conclusion, if understood as applying to the whole of the changes of position indicated by the comparison of the catalogues, was no doubt correct; but it is evident that, although the apparent displacements may not be capable of being completely explained on the hypothesis of a solar motion, it by no means follows that they do not in part depend upon this cause, and that, widely as the observed motions may differ in their relative directions, there may not still be a preponderance of motion, or a general tendency to move, towards some determinate point. LAMBERT, writing in 1761, remarked that the apparent changes in the positions of the fixed stars depend on the motion of the sun as well as on the motions of the stars themselves, whence, he says, "we may perhaps in time arrive at the means of determining towards what region of space the sun holds its course." The same philosopher also noticed that the rotation of the sun on its axis gives rise to a probability of its translation in space, although no proof can be given that the latter motion is a necessary consequence of the former.

The probability of the opinion that the observed proper motions of the stars are compounded of a real and an apparent motion was also noticed by MICHELL, who, in a note to a paper published in the Philosophical Transactions for 1767, remarked that the apparent change of situation which has been observed in a few of the stars, is a strong circumstance in favour of the opinion that those stars are among the nearest to us; and that the apparent displacement may be owing either to a real motion of the stars themselves, or to that of the sun, or partly to the one and partly to the other. And, he adds, "as far as it is owing to the sun's motion it may be regarded as a kind of secular parallax, which, if the annual parallax of a few of the stars should some time or other be discovered, and the quantity and direction of the sun's motion should be discovered also, might serve to inform us of the distance of many of them, which it would be utterly impossible to find out by any other means."

LAMBERT's argument, that the fact of the sun's rotatory motion about its axis affords a presumption of its translation in space, was adopted by LALANDE, who, in a memoir presented to the Academy of Sciences of Paris in 1776, concludes that inasmuch as the application of any force causing a body to turn about its centre cannot fail to displace the centre, the sun must necessarily have a real motion in absolute space. This argument will not be allowed to have much weight when it is considered that the sun's rotatory motion *may*, and probably does, proceed from causes wholly different from an eccentric impulsion.

Whatever degree of probability such *à priori* considerations may be supposed to give to the hypothesis of the sun's proper motion, it is evident that something more is necessary to render the hypothesis of any practical importance. The first astronomer who attempted to prove the existence of the sun's motion from observations,

and ventured to assign the precise point in the heavens towards which our system is actually borne, was Sir William Herschel. The paper containing this investigation was published in the Philosophical Transactions for 1783; and it is remarkable not only by reason of its giving the first determination of the kind, but on account of the confirmation which the result, though deduced from very insufficient data, has received from subsequent investigations—a circumstance, however, by no means rare in respect of the cosmical speculations of Sir W. Herschel.

After some general considerations respecting the *à priori* probability of the sun's proper motion, Sir W. Herschel, in the paper alluded to, proceeds to describe the phenomena to which it would give rise, namely, a general parallactic motion of the stars, the amount of which, in respect of any star, will depend both on the star's distance from the sun and its situation in the sphere with reference to the point towards which the sun is moving. It is manifest that if we suppose the sun to move in the direction of any assumed point, all the stars which are near enough to our system to be sensibly affected by such a motion, will appear to move towards the point diametrically opposite; and that on one side of the sphere all the right ascensions will appear to increase, while all those on the other side will appear to diminish. He selected seven stars—Sirius, Castor, Procyon, Pollux, Regulus, Arcturus, and α Aquilæ—all of which appeared from comparisons made by Dr. Maskelyne to have proper motions in right ascension, and two of them—Sirius and Arcturus—also in declination; and finding that the right ascensions of all of them, with the exception of α Aquilæ, appeared to diminish, he assumed the direction of the sun's motion to be from a point "somewhere not far from the 77th degree of right ascension to its opposite 257th degree," the effect of which would be to produce apparent changes of right ascension agreeing with the observed; and he adds, "supposing the sun to ascend at the same time towards some point in the northern hemisphere, for instance towards the constellation of Hercules, then will also the observed change of declination of Sirius and Arcturus be resolved into a single motion of the solar system." In order to test this conclusion he selected twelve stars, quoted by Lalande from Mayer's table above referred to, the motions of which were assigned both in right ascension and declination; and adding the motions in right ascension of three other stars, he thus obtained twenty-seven changes of position to be accounted for by the hypothesis. By assuming the sun's motion to be directed towards a point "somewhere near λ Herculis," he found that twenty-two of these motions were satisfied, there being only two exceptions in right ascension, and three in declination. The point thus indicated is situated at 257° of right ascension, and 25° of north declination; but he observes that with respect to the changes of declination the point λ Herculis is not, perhaps, the best-selected, as a somewhat more northern situation may agree better with the changes of declination of Arcturus and Sirius, "which capital stars," he thinks, "may be the most proper to lead us in this hypothesis."

In a *Postscript* to his paper, Sir W. Herschel compares the above conclusion with

the proper motions of the other stars in MAYER's table, and shows that, out of forty-four stars, the observed motions of thirty-two agree with the hypothesis, while those of the remaining twelve cannot be accounted for by it, and "must therefore be ascribed to a real motion of the stars themselves, or to some still more hidden cause of a still remoter parallax."

It will be remarked that the above result was arrived at without the aid of any calculation whatever, nor does it appear that the precise direction of the apparent motion of any of the stars was ascertained or taken account of. The author considered merely the changes in right ascension and declination, and gave such a direction to the solar motion as would produce corresponding changes in those two directions in the greatest number of instances, without reference to their relative amounts, or attempting to produce an exact coincidence of the hypothetical and apparent directions in any particular case. Nor did he pretend to assign the point towards which the sun's motion is directed with any precision; "it is somewhere near λ Herculis, but may be somewhat more to the north."

In the same year (1783) in which Sir W. HERSCHEL's paper appeared in the Transactions, PREVOST communicated the results of a similar inquiry to the Berlin Academy in a memoir which was published in the Nouveaux Mémoires of that Society for 1781. PREVOST's investigation was also grounded on the proper motions given in MAYER's table. After stating the opinion of MAYER that the observed motions could not be explained on the hypothesis of the motion of the solar system, he remarks, that on examining the table under every point of view, he had come to an opposite conclusion, and found that it did in fact afford indications of such a motion, although the true motions of the stars, or, perhaps, some other cause, occasioned exceptions. He then selects, from MAYER's list, twenty-six stars whose variations of position exceeded 14″ in right ascension or declination, and from a comparison of the whole concludes that the apparent motions indicated by the table would be most nearly represented by supposing the sun's motion to be directed towards that point of the heavens of which the right ascension is 230°, and the declination 25° north,—a conclusion which agrees with that of Sir W. HERSCHEL in respect of declination, but differs from it about 27° in right ascension. The agreement of the individual observations with this result he considered was sufficient to render it probable, first, that the solar system is actually moving towards the point indicated, and, secondly, that at the present time the stars which are the nearest to the sun are Sirius, Procyon and Arcturus; and he thought the observations also afforded grounds for conjecturing that the sun may be describing, *in antecedentia*, an orbit about Arcturus, or at least about a centre of gravity common to those brilliant stars which occupy the quarter of the heavens in which the right ascensions appear to diminish, such as Arcturus, Regulus, Procyon, Sirius*.

* Among the inferences drawn by PREVOST from the hypothesis of the sun's proper motion, the following may be remarked:—supposing comets to be formed of matter existing beyond our system, but projected so as

A third deduction from MAYER's table was made by KLUGEL, in the Berlin Ephemeris for 1789. After giving formulæ for determining from the observed variations in the positions of the stars the direction of the sun's motion in space, he applies them to the proper motions given in the table, and finds the pole towards which the sun's motion is directed to be at the point of which the right ascension is 260° and north declination 27°. This differs from Sir W. HERSCHEL's determination only by 3° of right ascension, and 2° of declination.

Although the general agreement of these three results was calculated to draw the attention of astronomers to the subject, and served, at least, to give a certain plausibility to the hypothesis, no further addition was made to the data of the problem till the publication, in 1790, of Dr. MASKELYNE's table of the proper motions of thirty-six stars. This table, which furnished much more certain data than had previously existed, gave occasion to a second elaborate memoir by Sir WILLIAM HERSCHEL, which appeared in the Philosophical Transactions for 1805.

The mode of proceeding employed by Sir W. HERSCHEL in this memoir merits attention. Having computed from the observed variations of right ascension and declination the apparent direction of the proper motion of each of the stars, he traced on a celestial globe the great circles in which they were contained. On the supposition that the variations in question were parallactic motions caused by the translation of the sun, it was evident that all the great circles containing them would intersect each other in the same two opposite points of the sphere. Now of the intersections thus formed by taking the stars in pairs, he found ten made by six stars of the first magnitude to be contained within a very limited portion of the heavens about the constellation Hercules, while (he remarks) "upon all the remaining surface of the globe there was not the least appearance of any other than a promiscuous situation of intersections, and of these only one was made by arches of principal stars." The six stars which gave the contiguous intersections were Sirius, Arcturus, Capella, Lyra, Aldebaran and Procyon. But six stars combined by pairs give fifteen intersections; of these, therefore, five were rejected, that is to say a third of the whole, as not agreeing with the hypothesis. He then computes, by a trigonometrical calculation, the exact situations of the points of intersection of the ten arches, and (taking the points from which the stars appeared to recede) found them to be all included between 235° and 290° of right ascension, and between 17° and 58° of north declination. He then takes into account the motions of three other large stars, of the second order, and, on combining them with those of the former six, found out of the whole number of new intersections fifteen which agreed with the former in "pointing out the same part of the heavens as a parallactic centre." The positions of these fifteen new points were not calculated, but determined graphically; he conceived, however, they might be depended on as true to one degree of the sphere.

to come within the sphere of the sun's attraction, ought it not (he asks) to happen that more comets will appear in the quarter of the heavens towards which the sun is advancing than the opposite quarter?

The intersections thus found, although lying in the same quarter of the heavens, were not confined within a very narrow space, and in order to obtain a precise result, he proceeds as follows. Confining his attention to the six stars above named, he found the sum of their annual apparent motions in space to be 5″·3537. Now, assuming the star λ Herculis (as determined in his first paper) to be the point towards which the sun is moving, he computes the angle included between the great circle of the sphere which passes through this point and the star, and the great circle in which the star's motion takes place according to the comparison of the catalogues; he then multiplies the apparent quantity of the annual proper motion of the star by the cosine and sine respectively of this angle, whereby the apparent proper motion is resolved into two parts,—one in the direction in which the star would appear to move in consequence of the hypothetical motion of the sun, and the other at right angles to that direction. The first of these may be ascribed to the motion of the sun; the second must be regarded as due to the true proper motion of the star. Adding, therefore, into one sum the former of these resolved parts for each of the six stars, and deducting the sum from the sum of the observed annual motions, the latter sum was reduced from 5″·3537 to 2″·2249. By assuming another point in the same constellation as the apex of the sun's motion, the sum of the annual proper motions of the six stars, in the direction perpendicular to that resulting from the hypothesis, was further reduced to 1″·4594; and after some other trials, he ultimately fixed upon the point (near 34 Herculis) whose right ascension was 245° 52′ 30″, and north declination 49° 38′, by which the sum of the true annual proper motions of the six stars was reduced to 0″·9559. He concluded that this point must be very near the truth, inasmuch as "the alteration of a few minutes in right ascension or north polar distance, either way, would immediately increase the required real motion of our stars."

This determination of the position of the solar apex differs from that which was given in Sir W. Herschel's former paper by about 11° of right ascension, and 24° 38′ of declination. It rests, however, on the proper motions of only six stars, and, therefore, notwithstanding the greater probable accuracy of the observations, and the more elaborate process of calculation by which it was arrived at, it is probably not of greater intrinsic value than the first. The principle on which it is based, namely, the supposition that the sum of the true proper motions of the stars is a minimum, and consequently that the direction to be assigned to the sun's motion must be that which will account for the greatest amount possible of the observed motions, was objected to by Burkhardt, on the ground that there is no more reason for supposing the sum of the true proper motions to be a minimum than a maximum, excepting on the hypothesis that the stars are more inclined to rest than to motion. But this objection seems to imply some misapprehension of the problem under consideration. No hypothesis respecting the disposition of the stars to rest or to motion is involved. The apparent proper motions are the results of the comparison of the catalogues, and

the question proposed by Sir W. Herschel was simply to determine the point towards which the sun must be supposed to move, in order that, after deducting the parallactic effect, the amount of the residual motions might be the least possible. Burkhardt's memoir was published in the Connaissance des Temps for 1809. It contains formulæ for the solution of the problem, with their application to several of the stars in Maskelyne's catalogue; but he found little accordance among the results, and concluded that we are not yet in possession of a sufficient number of facts to decide on the direction of the sun's motion.

Biot, in the Additions to his Astronomie Physique, also considered the question of the translation of the planetary system, and gave formulæ for determining the right ascension and declination of the solar apex. He computed the intersections of the great circles containing the arcs described by eight stars, viz. Aldebaran, Capella, Sirius, Procyon, Pollux, Arcturus, α Lyræ, and α Aquilæ, the proper motions of which were given by Zach from a comparison of Bradley's places with those of Maskelyne, and also of the catalogues of Mayer and Piazzi. If the apparent motions depended solely on the displacement of our system, the intersections would, of course, all be found at the same two points of the sphere; but he found the discrepancies to be so great that he considered them to be irreconcileable with the supposition of their dependence on any systematic motion or common cause. "The examination," he remarks, "of all these irregularities shows that the stellar motions hitherto observed are not subject to any law, and that it would be in vain to attempt to reconcile them by supposing them all to be directed towards the same pole. Hence it becomes infinitely probable that such of these motions as are well determined are due, in part, to a real displacement of the stars themselves, and not to that of our system. With respect to those whose proper motions are less certain, not only does their want of precision prevent us from concluding from them the direction of the motion of our own system, but their comparison does not even afford any indication which can lead to the inference that it is in motion at all."

In the 12th section of his Fundamenta Astronomiæ, Bessel has given an elaborate investigation of this question, founded on a much larger number of proper motions (and probably better determined) than had previously been brought to bear on the inquiry. On comparing the catalogue deduced from Bradley's observations with that of Piazzi, he found seventy-one stars having a proper motion of not less than $0''{\cdot}5$ annually in the arc of a great circle, and computed the positions of the great circles in which the apparent motions are contained. But even from this large number he obtained no satisfactory or conclusive result. The investigation, he remarks, did not confirm Herschel's supposition of the sun's motion towards the constellation Hercules, since many points on the sphere, very remote from each other, and even diametrically opposite, may be assigned which are situated in the direction of the motion of many stars; but whatever point may be taken, there will always be found so many proper motions evidently receding from it, that no sufficient reason

will remain for preferring one point to another. And he concluded that a very long time must elapse before any proficiency would be made in the theory of the proper motions of the stars.

The opinion of BESSEL, now quoted, appears to be that which, until lately at least, has been generally entertained by astronomers; but on attentively considering the nature of the question, it will soon be seen that none of the methods of investigation yet alluded to can be considered as capable of leading to an entirely satisfactory conclusion. They are all founded essentially on the principle of determining the apex of the sun's motion from the apparent motions of single pairs of stars; and, with the exception of BESSEL'S, all the results which had been given were deduced from a very small number of proper motions. Now, it is a very improbable supposition that the stars are subject to no variations but such as depend on the motion of the sun. We must suppose them to have true proper motions, producing apparent effects at least equal in amount to those which are supposed to be produced by the sun's displacement. Assuming, then, that the stars themselves are in motion as well as the sun, and that they move in all directions, the appearances will necessarily be of a very complicated nature. The proper motions of some stars will conspire with that of the sun, and increase the apparent change of position. In other cases they will be contrary to that of the sun, and the apparent effect will be that which is due to the difference of two real motions. In general the directions of the true and parallactic motions will be inclined to each other; but in all cases the difference given by the comparison of the catalogues will be compounded of the effect of the real motion of the star and the effect of the sun's displacement. Hence it is manifest that the fact of proper motions being observed to take place in all directions, is in no way inconsistent or incompatible with an apparent general drifting of the stars towards one particular region; and the problem to be solved is to separate, if possible, the general effect produced by the sun's displacement from the complicated effects caused by the motions of stars in every direction with which it is entangled and mixed up. Now it is easy to see that a question of this kind cannot be solved by taking account of only a small number of proper motions. A very considerable number must be employed; and, indeed, in order that the solution may be satisfactory, regard must be had to every star without exception of which the proper motion has been determined with sufficient certainty. It is also necessary that the investigation be conducted in such a manner that every observed displacement shall contribute, according to its weight, to the general result; and the probable error of the result must likewise be determined in order that the relative probabilities of results obtained from different hypotheses respecting the direction of the sun's motion, may be submitted to exact comparison. In this manner it will be seen whether, as BESSEL and others inferred from particular cases, numerous points may be assumed towards any one of which the sun may be supposed with equal probability to be advancing, or whether there is so great a preponderance of observed motions towards one particular region

as to warrant the assumption of a systematic origin, or make evident the operation of a common cause. This mode of considering the question was first adopted by Argelander, in the memoir alluded to at the commencement of this paper.

Argelander's investigation, as already stated, is founded on the proper motions of 390 stars, determined by a comparison of their mean places in 1755, according to Bessel's reduction of Bradley's observations, with their mean places in 1830, as given in his own catalogue*, deduced from observations made by himself at Abo, the interval between the epochs being seventy-five years. In this investigation every star was included which appeared, on comparison of the two catalogues, to have undergone a change of position to the extent of $7''{\cdot}5$, or to have an annual proper motion amounting to $0''{\cdot}1$ in space. By reason of the excellence of both catalogues, the long interval between their respective epochs, and the very considerable number of stars employed, the result must be considered as by far the most satisfactory that had yet been given.

The method of calculation employed by Argelander may be described generally as follows:—1. From the variation in right ascension and declination given by the comparison of the catalogues, the angle (ψ) is computed which the apparent path described by the star makes with the circle of declination. 2. A point (Q) is assumed as the apex of the sun's motion, and the direction in which the star would appear to move (if it had no real motion of its own) in consequence of the motion of the sun, is computed from the position of the star and the assumed position of the point Q, and expressed in terms of the angle (ψ') which it makes with the declination circle. 3. The trigonometrical value of ψ' is differentiated on the supposition that the right ascension (A), and declination (D) of the point Q are variable quantities, and in the resulting expression the numerical value of the difference of the angles ψ and ψ' is substituted for $d\psi'$, by which means an equation is obtained in which there are only two undetermined quantities, viz. dA and dD. Each star furnishes a similar equation; and as the effect of the real motion of the sun on the apparent displacement of any star is proportional to the sine of the star's distance from the apex of the sun's motion, each equation is multiplied by the corresponding sine of this distance, whereby they are all reduced to the same degree of precision. 4. The equations are then solved by the method of least squares, and the resulting values of dA and dD applied as corrections to the assumed values of A and D, which determine the situation of the point Q. With the corrected values of A and D thus obtained, the angles ψ' may be recomputed, and after one or two repetitions of the same process, values of A and D will be obtained giving the position of Q which most nearly represents the whole of the observations.

The effect of the sun's displacement on the apparent proper motion of a star is inversely proportional to the distance of the star from the sun; but this distance

* DLX Stellarum fixarum positiones mediæ ineunte anno 1830. Ex observationibus Aboæ habitis deduxit ..... Fr. Argelander. Helsingforsiæ, 1835. 4to.

being entirely unknown, no account can be taken of it excepting upon some assumption more or less arbitrary. ARGELANDER assumes as a probable hypothesis, that those stars which have the largest proper motions are the nearest to our system, and introduces the condition of relative proximity by dividing the stars upon which his calculation was made into three classes, and giving different weights to the equations belonging to the different classes, all the stars in the same class being assumed to be at the same mean distance. The first class contained twenty-one stars, having proper motions exceeding one second of arc annually; the second contained fifty stars whose annual proper motions are between 1″·0 and 0″·5; and the third the remaining 319 stars, the annual proper motions of which were included between 0″·5 and 0″·1. The partial results deduced from each class presented a nearer agreement than was, perhaps, to be anticipated from the nature of the question. In giving an account of his memoir in No. 363 of the Astronomische Nachrichten, ARGELANDER corrects some errors of calculation which had escaped detection in the original paper, and states the most probable values (with their probable errors) of the right ascension and declination of Q, as resulting from the combination of the whole of the equations of condition, to be as follows:—

$$A=259^\circ\ 47'\cdot6\pm3^\circ\ 18'\cdot6,\quad D=+32^\circ\ 29'\cdot5\pm2^\circ\ 13'\cdot5,$$

for 1792·5 (the mean epoch of the catalogues), or

$$A=259^\circ\ 51'\cdot8,\quad D=+32^\circ\ 29'\cdot1,$$

when reduced to the beginning of 1800.

This result differs very considerably from that which was obtained by Sir W. HERSCHEL in his paper of 1805, viz. $A=245^\circ\ 52'\ 30''$, $D=+49^\circ\ 38'$, but approximates nearly to the determination in the paper of 1783; the difference from the latter being less than 3° in right ascension, and about $7\frac{1}{2}^\circ$ in declination.

In order to give an idea of the probable accuracy of this result, ARGELANDER deduces the following conclusions. If with the point Q thus found as a centre, and a radius containing 3° 45′·7, a circle be described on the sphere, the wager is 1 to 1 that the sun's motion is directed to some point within this circle; 14 to 3 that it is directed to some point within a circle having the same centre and a radius of 7° 31′·4; 89 to 4 that it is directed to a point within a circle having the same centre and a radius of 11° 17′·1; 142 to 1 that the point will be within a circle having the same centre and a radius containing 15° 2′·8; and if we increase the radius to 18° 48′·5, the wager will be more than 1341 to 1 that the point Q will lie somewhere within that circle.

In No. 398 of the Astronomische Nachrichten, ARGELANDER returns to the subject a third time, and gives another determination of the direction of the solar motion, calculated by LUNDAHL from a different set of stars. The Abo catalogue does not contain the whole of BRADLEY's stars given in the Fundamenta Astronomiæ, and on comparing the latter work with POND's catalogue of 1112 stars (reduced to the begin-

ning of 1830), LUNDAHL found 147 not included in ARGELANDER's investigation, whose proper motions appeared to be not less than 0″·09 of space annually. Having first recomputed the places of those stars with a more exact value of the precession, and applied to POND's observations the correction necessary to render them strictly comparable with those of ARGELANDER, LUNDAHL assumed the apex of the sun's motion to be situated at the point indicated by ARGELANDER's investigation, and calculated the value of ψ' for each star on this assumption. Comparing the directions thus obtained with those of the apparent motions, and forming the equations of condition according to the method of ARGELANDER, the resulting values of the right ascension and declination of Q were found to be

$$A=252^\circ\ 24'\cdot4\pm5^\circ\ 25'\cdot3\ ;\quad D=+14^\circ\ 26'\cdot1\pm4^\circ\ 29'\cdot3.$$

This result differs from that of ARGELANDER more than 8° in right ascension, and about 17° in declination; and the corrections of the assumed values are far beyond the limits of the probable errors assigned by ARGELANDER. By reason, however, of its smaller weight, it does not, when combined with the former determinations, materially alter the probable situation of the point Q. Combining it with the results of each of his three classes, with due regard to their relative weights, ARGELANDER gives the following values of the coordinates of Q as the most probable result of the whole of the observations:—

$$A=257^\circ\ 49'\cdot7\pm2^\circ\ 49'\cdot2\ ;\quad D=+28^\circ\ 49'\cdot7\pm1^\circ\ 59'\cdot8.$$

A still more recent attempt to assign the position of the apex of the sun's proper motion has been made by OTTO STRUVE, the results of which are given in a paper published in the Petersburg Memoirs (tome 5) for 1842. This investigation is grounded on the proper motions of about 400 stars, as determined by a comparison of their mean places in 1755, according to BESSEL's catalogue, with their positions in 1825 deduced from observations made at the Dorpat Observatory. Of the whole number of stars employed, only 134 are included among those from which ARGELANDER's result was deduced, so that about 260 additional proper motions are brought to bear on the hypothesis. The mode of investigation is different in several respects from that which has been described. Assuming the direction of the sun's proper motion to be determined, the proper motions indicated by the comparison of the catalogues are manifestly functions of the constant of precession used in reducing BRADLEY's places to 1825, and of the quantity of the solar motion. From the equations of condition furnished by the observed variations of right ascension and declination, he determines the precession and the angular motion of the sun (which, as seen from the mean distance of stars of the first magnitude, he finds to be 0″·339 in a year, with a probable error of 0″·025), and having substituted these values in the equations of condition, he employs the residual errors in forming a new system of equations which serve to determine dA and dD, the corrections in right ascension and declination of the assumed direction of the solar motion. The directions of the

apparent proper motions were not computed, but the variations in right ascension and declination expressed separately in terms of the assumed values of A and D and the star's place, so that each star furnishes two independent equations. For determining the relative weights of the equations, he adopts the hypothesis that the distances of the stars are inversely as their apparent magnitudes; and, dividing all the stars from the first to the seventh magnitude inclusive into twelve classes, he assumes, on grounds given by the elder STRUVE, in the Introduction to his catalogue of double stars, the mean distance of those in the first class = 1, of those in the second = 1·71, and so on to the twelfth class, or seventh magnitude, for which the mean distance becomes 11·34. Weights depending on those distances were assigned to the equations furnished by the stars in each of the twelve classes, and the values of dA and dD deduced. The result gave the position of Q, for 1790, as follows:—

$$A = 261^\circ\ 21'{\cdot}8 \pm 4^\circ\ 49'{\cdot}9\,;\quad D = +37^\circ\ 36'{\cdot}0 \pm 4^\circ\ 11'{\cdot}8.$$

For the sake of comparison, I here subjoin the results of the several investigations of ARGELANDER, LUNDAHL, and OTTO STRUVE.

Position of the apex of the sun's proper motion for 1792·5,

		A=	D=	
ARGELANDER	I.	256° 25′·1 ± 12° 21′·3	+38° 37′·2 ± 9° 21′·4	(21 stars).
ARGELANDER	II.	255° 9′·7 ± 8° 34′·0	+38° 34′·3 ± 5° 55′·6	(50 stars).
ARGELANDER	III.	261° 10′·7 ± 3° 48′·9	+30° 58′·1 ± 2° 31′·4	(319 stars).
LUNDAHL	IV.	252° 24′·4 ± 5° 25′·3	+14° 26′·1 ± 4° 29′·3	(147 stars).
O. STRUVE	V.	261° 23′·1 ± 4° 49′·9	+37° 35′·7 ± 4° 11′·8	(392 stars).

From these five determinations O. STRUVE deduced the following mean result:—

$$A = 259^\circ\ 9'{\cdot}4, \text{ with a probable error } = 2^\circ\ 57'{\cdot}5.$$
$$D = +\ 34^\circ\ 36'{\cdot}5, \text{ with a probable error } = 3^\circ\ 24'{\cdot}5.$$

The proper motions on which the following investigation is grounded are deduced from a comparison of the mean positions of eighty-one stars in the southern hemisphere, as observed by Mr. JOHNSON and the late Professor HENDERSON, with the positions assigned to them in the catalogues of LACAILLE and BRADLEY. Every star, without exception, has been included, which, from the differences of right ascension and declination given in the two recent catalogues, appears to have a proper motion amounting to 0″·1 in space, or upwards, annually.

Mr. JOHNSON's catalogue, which was published in 1835, gives the mean positions of 606 stars, observed by him at St. Helena, and reduced to the beginning of 1830. Of these stars a considerable number are contained in LACAILLE's catalogue, originally published in the Astronomiæ Fundamenta (1757), and recently by Mr. BAILY in vol. v. of the Memoirs of the Royal Astronomical Society. The epoch of LACAILLE's catalogue is 1750, so that the interval is eighty years. In order to compare the two catalogues, Mr. JOHNSON reduced the positions given by LACAILLE to the epoch 1830, by applying

the precession due to the middle epoch, 1790; and on examining the proper motions of the stars so compared, I have found fifty-six which appear to have changed their positions 8″·0 or upwards in the arc of a great circle, or to have an annual proper motion of not less than 0″·1. To these have been added five others whose annual proper motions appear to be somewhat less than 0″·1, according to Mr. Johnson's observations, but to amount to that quantity according to Henderson's determination compared with that of Lacaille. The whole number of stars, therefore, included in the present inquiry, whose proper motions are deduced from the comparison of the observations of Mr. Johnson with those of Lacaille, is sixty-one.

Henderson's catalogue contains the mean right ascensions and declinations of 172 of the principal southern stars, being part of a very much larger number observed by him during his short residence at the Cape in 1830 and 1831, while in charge of the Government Observatory established in that colony, the reduction of which unfortunately he did not live to complete. The declinations were published in 1838, in vol. x. of the Memoirs of the Royal Astronomical Society, and the right ascensions in vol. xv. of the same series, which appeared only last year (1846). In the latter volume he gives a list of fifty-two stars which appear to have a proper motion of not less than 0″·1 annually, deduced in thirty-six cases from a comparison of his own observations with those of Lacaille, and in the remaining sixteen cases with those of Bradley. Henderson's mean places are for the beginning of 1833; so that the interval is eighty-three years in the case of comparison with Lacaille, and seventy-eight years in the case of comparison with Bradley. The whole of the thirty-six stars compared with Lacaille, are contained in Mr. Johnson's catalogue, but there are four of them in respect of which the comparison is not given by Mr. Johnson, and which, therefore, are not included among the sixty-one above referred to. Henderson's catalogue, therefore, gives twenty additional stars, so that on the whole the number taken into account is eighty-one; namely, sixty-five whose proper motions depend on Lacaille's observations, and sixteen on the observations of Bradley.

For the purpose of deducing the direction of the apparent proper motion from the observed variations of right ascension and declination, as well as for determining the hypothetical direction, every star has been referred to its mean position for 1790. In the case of Mr. Johnson's stars, this reduction has been made by taking the mean right ascension and the mean declination of the two compared catalogues. In respect of the stars in Henderson's catalogue, those which are compared with Lacaille's places were first reduced to 1830 by applying the precession given in the catalogue, and the mean then taken between these reduced places and the places of Lacaille; and in the case of those compared with Bradley's catalogue, the mean of the two catalogues gave the positions for 1794, from which they were reduced to 1790, by applying the precession for that epoch. With respect to the stars common to the catalogues of Mr. Johnson and Henderson, the annual variation both in right ascen-

sion and declination was first deduced from the comparison of each catalogue with LACAILLE's places, and the mean of the two comparisons then taken and made use of in the subsequent calculations.

Having thus obtained the places of the stars for the mean epoch 1790, and the annual variations in right ascension and declination being found by dividing the differences of the catalogues (expressed in seconds of arc) by the number of years in the interval, the angle ψ, which the apparent path of the star makes with the circle of declination, was computed. This angle determines the position of the great circle of the sphere in which the apparent motion takes place.

The next step in the process is to compute the angle ψ', or the direction in which the star would appear to move in consequence of the translation of the sun towards an assumed point Q. With respect to this point, or apex, the position which may be regarded as the most probable is, perhaps, that which was deduced by OTTO STRUVE from the five determinations above given; but the object here was not to choose the point which has the greatest probability in its favour, but that which appeared the most likely to satisfy the present observations. Now, on examining the apparent motions of the stars under consideration, it was easy to see that the apex must have a considerably greater declination than that which was assigned to it by LUNDAHL. OTTO STRUVE's result, on the other hand, which differs from the mean in the opposite direction, appeared to me to be less trustworthy from the manner in which it was deduced. I therefore assumed, as the apex of the solar motion, the point determined by ARGELANDER, though, as it turns out, the motions in declination would have been somewhat better satisfied by assuming the mean of all the results as given by OTTO STRUVE. Its position for 1790 (the mean epoch of the catalogues) is

$$A = 259^\circ\ 46'{\cdot}2,\quad D = +32^\circ\ 29'{\cdot}6*.$$

From these values of A and D the angle ψ' was computed for each of the eighty-one stars separately. The results, as well as the values of ψ, and the differences $\psi - \psi'$, will be found in a table hereto subjoined.

Before proceeding to state the results obtained from the equations of condition, it will be worth while to examine the presumptions for or against the hypothesis deducible from the comparison of the directions of the apparent proper motions of the different stars, and the directions of the parallactic motions which would result from the motion of the sun towards the assumed point Q.

First, with respect to the observed variations of right ascension: if we conceive the celestial sphere to be divided into two hemispheres by a great circle passing through

* This is the position according to the values of the right ascension and declination given by ARGELANDER in No. 363 of the Astronomische Nachrichten, but in a subsequent number of the same work (No. 398) it is stated that an error of calculation had been committed the correction of which would have given the position of Q for 1792·5 as follows: $A = 260^\circ\ 51'$, Dec. $= +31^\circ\ 17'$. The correction was not observed till after the values of ψ' had been calculated for all the stars; but for the present purpose the difference is manifestly of no importance.

Q and perpendicular to the equator, the effect of the sun's motion towards Q would be to increase the right ascensions of all the stars in one of the hemispheres, and to diminish the right ascensions of all those in the other. Now, out of the whole of the stars compared, thirty-five are situated in the hemisphere in which the right ascensions should increase according to the hypothesis, and of these there are twenty-three whose right ascensions have actually increased, and twelve whose right ascensions have diminished; that is to say, there are twenty-three instances favourable to the hypothesis, and twelve unfavourable. In the other hemisphere there are forty-six stars; and of these the number whose right ascensions have diminished, agreeably to the hypothesis, is thirty-nine, and the number whose right ascensions have increased, contrary to the hypothesis, is seven. Hence it appears that in respect of the eighty-one stars included in the inquiry the observed motions in right ascension are favourable to the hypothesis in sixty-two instances, and unfavourable in nineteen. Allowing the same weight to each instance, the wager is therefore sixty-two to nineteen, or somewhat more than three to one in favour of the hypothesis of a common tendency towards a determinate region.

Secondly, with respect to the observed variations of declination. In all cases in which the angle ψ' is less than 90°, or greater than 270°, the effect of the sun's motion in the direction of the assumed point Q, is to bring the apparent place of the star towards the north, so that its declination (which is south in all cases) should appear, on comparison of the catalogues, to have diminished; and in all cases in which ψ' is greater than 90° and less than 270°, the variation in the place of the star should be towards the south, and the declination should increase. Now, in the first case there are fifteen stars, and of these ten have advanced towards the north, agreeably to the hypothesis, and five towards the south, contrary to the hypothesis. In the second case there are sixty-six stars; and of these the observed motion of fifty-three is towards the south, agreeing with the hypothesis, and of thirteen towards the north, contrary to the hypothesis. On the whole, therefore, in respect of declination, there are sixty-three instances favourable to the hypothesis and eighteen unfavourable; and the wager is seven to two in favour of the hypothesis. It may be added that there are only three stars out of the whole number (Nos. 18, 34 and 36 in the subjoined table) whose observed proper motions are contrary to the hypothesis both in right ascension and declination.

Another inference may be drawn from the comparison of the angles ψ and ψ'. If the observed changes of position were wholly independent of the sun, all directions would be equally probable, and it would be an even wager that the difference between ψ and ψ' would be less or greater than 90° in any case, since all possible values of that difference lie between 0 and 180°. But there are only ten instances, out of eighty-one, in which the difference exceeds 90°.

From this general agreement of the hypothetical and observed motions, a strong presumption is raised in favour of the hypothesis; for it can scarcely be supposed.

that the agreement would hold good in so great a majority of instances if it were purely the effect of chance. But a much more certain conclusion will be arrived at from the combination of the whole of the observations by the method of least squares.

The method of forming the equations of condition has already been explained generally, and the formulæ for computation will be given in subsequent paragraphs; but as some of the stars are more favourably circumstanced than others for determining the question at issue, it becomes necessary, before proceeding with the solution, to assign weights to the equations, in order to reduce them all to the same precision, and obtain the most probable values of the corrections to be applied to the assumed position of the solar apex. For this purpose some special considerations are required.

Admitting the hypothesis of the sun's motion, it can hardly be supposed that any star is absolutely at rest. The apparent motion of a star, therefore, as it is made known to us by a comparison of observations, is the effect of the combined motion of the sun and the star. Now, with respect to the true proper motion, we are in ignorance of all the circumstances by which its apparent or visible effect is modified. We know nothing whatever respecting the magnitude or nature of the orbit described by the star, or the absolute velocity with which it moves. Hence it is necessary to assume that all the stars move with the same absolute velocity, in which case (putting the sun's motion out of consideration) the apparent velocity will be inversely as the distance. But we are equally ignorant of the relative distances, and are therefore reduced to the necessity either of disregarding the distance altogether, or of making some precarious assumption respecting it,—for instance, that the distances of the different stars are inversely proportional to their magnitudes (as in the method of Otto Struve), or inversely as the quantities of the apparent proper motions. In the present inquiry no greater probable accuracy could be obtained by the adoption of either of these assumptions, and consequently errors to which differences of distance as well as of absolute velocity give rise, are regarded as constant. The only remaining circumstance by which the apparent effect of the true proper motion is modified is its direction; and as there is no *à priori* reason for assuming that a star is more likely to move in one direction than another, all directions must be regarded as equally probable. The conclusion, therefore, is, that in respect of the true proper motions *inter se*, we have no sufficient grounds for making any distinction as to the relative precision of the results given by different stars, so that the errors arising from this cause must be treated as accidental errors of observation, and all the equations be allowed to have the same weight.

With respect to the part of the apparent motion depending on the displacement of the sun, the case is different, inasmuch as the parallactic effect depends not only on the distance of the star, but also on its situation with respect to the apex of the sun's motion. The effect of the sun's motion on the observed position of a star (as will be shown more particularly further on) is directly as the sine of the star's distance from

the point Q towards which the sun is moving; and hence, in order that all the equations may have the same weight, each must be multiplied by the sine of that distance. In other words, if $\varepsilon(\Psi)$ denote the probable error in the observed direction of the proper motion, or the probable value of $\psi-\psi'$, for a star at the distance of 90° from the point Q, then $\varepsilon(\Psi)$ will also be the probable value of $(\psi-\psi')\sin\chi$ for a star whose distance from Q is measured by the angle χ. Hence it follows that every value of $\psi-\psi'$ must be multiplied by $\sin\chi$.

The eighty-one equations of condition are given in an appended table. They are of the following form,

$$0=+ad\mathrm{A}+bd\mathrm{D}-n,$$

where a, b, and n are numbers deduced from the data, and dA, dD the quantities to be determined from the equations and applied as corrections to the assumed values of A and D. Forming the squares and products of these numbers, and adopting, according to the usual notation, (aa) to denote the sum of the squares of the coefficients of dA, (bb) the sum of the squares of b, (ab) the sum of the products of a and b, and so on, the following values are found:—

$$(nn)=178660{\cdot}4,\ (aa)=38{\cdot}5423,\quad (bb)=26{\cdot}7425,$$
$$(ab)=-5{\cdot}6852,\ (an)=-129{\cdot}462,\ (bn)=-105{\cdot}693,$$

and consequently the two following equations for determining dA and dD, viz.

$$0=+38{\cdot}5423d\mathrm{A}-\ 5{\cdot}6852d\mathrm{D}-129{\cdot}462,$$
$$0=-\ 5{\cdot}6852d\mathrm{A}+26{\cdot}7425d\mathrm{D}-105{\cdot}693,$$

the solution of which gives

$$d\mathrm{A}=+4^{\circ}{\cdot}070 \text{ with the weight } 37{\cdot}333,$$
$$d\mathrm{D}=+4^{\circ}{\cdot}817 \text{ with the weight } 25{\cdot}904.$$

On computing $\varepsilon(\Psi)$, or the probable value of $(\psi-\psi')\sin\chi$, from the appropriate formula of the method of least squares, we find $\varepsilon(\Psi)=31^{\circ}{\cdot}98$; whence, and from the above weights, the probable errors of dA and dD are respectively $5^{\circ}{\cdot}234$ and $6^{\circ}{\cdot}285$. The result, therefore, of the whole calculation from the assumed values of A and D, namely $\mathrm{A}=259^{\circ}\ 46'{\cdot}2$, $\mathrm{D}=+32^{\circ}\ 29'{\cdot}6$, gives the following values of A and D for the position of the point Q for the beginning of 1790,

$$\mathrm{A}=263^{\circ}\ 50'{\cdot}4\pm5^{\circ}\ 14'{\cdot}0;\quad \mathrm{D}=+37^{\circ}\ 18'{\cdot}6\pm6^{\circ}\ 17'{\cdot}1.$$

This result presents a very remarkable agreement with that obtained by Otto Struve from the Dorpat observations; the values of dA and dD are, however, somewhat greater than the probable errors of the hypothesis, according to the determination of Argelander.

Following out the principle of the method, the next step would be to recompute the angles ψ', and the equations of condition, with the values of A and D now found, so as to obtain a result having a smaller probable error; but, in the present case, the labour attending a new calculation (by no means inconsiderable) is altogether unnecessary, as will appear from the following considerations.

In the first place it is to be remarked, that the result has been deduced from all the stars whose annual proper motions were found to be not less than 0″·1, without any selection or rejection on account of obvious discrepancies. But it is manifest that if there is observed a general tendency to motion in a determinate direction, while in one or two instances the motion is in a nearly opposite direction, the presumption will be that in such exceptional cases the disagreement arises from the circumstance that the parallactic motion is masked and concealed by the relatively greater proper motion of the star. In the second place, a few of the stars under consideration are very unfavourably situated for correct determination of the right ascension according to the method practised by Lacaille, and also for correct comparison by reason of the uncertainty of the computed precession; and in such cases a disagreement with the general result naturally gives rise to a suspicion of error in the determination. Now there are two stars, β and γ^1 Octantis (Nos. 15 and 18 in the table) which are in those circumstances. The difference between the observed and hypothetical directions of their motion, or $\psi - \Psi$, is 158° 58′·0 in the case of the first, and 179° 13′·0 in that of the second, showing in the latter case an apparent motion almost directly opposite to the parallactic motion due to the hypothesis. Both stars are also situated within 8° of the pole, so that the determination of their right ascensions by Lacaille's method of equal altitudes must be liable to considerable uncertainty. For these reasons it may be concluded that the probable accuracy of the result will be increased by rejecting those two stars from the calculation.

Omitting, therefore, the two equations 15 and 18, the sums of the squares and products of the numerical quantities in the remaining seventy-nine are as follows:

$$(nn) = 137120{\cdot}9,\ (aa) = 37{\cdot}1490,\quad (bb) = 26{\cdot}7010$$
$$(ab) = -5{\cdot}9259,\ (an) = +110{\cdot}849,\ (bn) = -64{\cdot}282;$$

which give the following solution,

$$dA = -\ 2^\circ\ 41'{\cdot}8 \pm 4^\circ\ 44'{\cdot}9,$$
$$dD = +\ 1^\circ\ 48'{\cdot}5 \pm 5^\circ\ 36'{\cdot}0$$
$$\varepsilon(\Psi) = \ 28^\circ\ 25'{\cdot}2.$$

Here the corrections, both in right ascension and declination, are considerably diminished, and it will be remarked that the former has changed sign and become subtractive instead of additive. The probable errors are also considerably reduced, and both corrections are now within the limits of the probable errors of Argelander's determination.

On applying the above corrections to the assumed values of A and D, we obtain, as the result of the calculation from seventy-nine stars,

$$A = 257^\circ\ 4'{\cdot}4 \pm 4^\circ\ 44'{\cdot}9,\ D = +34^\circ\ 18'{\cdot}1 \pm 5^\circ\ 36'{\cdot}0.$$

Another omission will diminish the corrections and probable errors still further. One of the stars in Henderson's catalogue (η Ophiuchi, No. 80 in the subjoined table)

appears to move in a direction nearly opposite to that of the parallactic motion resulting from the assumed hypothesis, the difference of the angles ψ and ψ' being in this case 173° 4′·9. If we reject this star also, on account of the great probability there is that the apparent motion is here due to the excess of the true proper motion of the star above the parallactic motion, we shall have from the remaining seventy-eight stars,

$$(mm)=120332{\cdot}5,\ (aa)=36{\cdot}4473,\ (bb)=26{\cdot}6875,$$
$$(ab)=-5{\cdot}8285,\ (an)=+2{\cdot}308,\ (bn)=-49{\cdot}213,$$

whence the following results,

$$d\mathrm{A}=+0^\circ\ 14'{\cdot}4\pm4^\circ\ 31'{\cdot}4,$$
$$d\mathrm{D}=+1^\circ\ 53'{\cdot}8\pm5^\circ\ 17'{\cdot}2,$$
$$s(\Psi)=\ 26^\circ\ 49'{\cdot}8.$$

Applying these corrections to the assumed values of A and D, the position of the point Q, for 1790, is found as follows :—

$$\mathrm{A}=260^\circ\ 0'{\cdot}6\pm4^\circ\ 31'{\cdot}4,\quad \mathrm{D}=+34^\circ\ 23'{\cdot}4\pm5^\circ\ 17'{\cdot}2.$$

These values of A and D are almost identical with those which were deduced by Otto Struve from the combination of his own result with those of Argelander and Lundahl.

From the near agreement of these results with the hypothesis, it is manifest that it would be an entirely useless labour to recompute values of ψ' from slightly altered values of the right ascension and declination of Q, the corrections of the assumed values being so far within the limits of the probable errors. So close a coincidence, whether accidental or otherwise, is not a little remarkable. In fact the southern stars would seem to accord with the hypothesis even better than those in the other hemisphere ; for the mean value of $(\psi-\psi')\sin\chi$, or $s(\Psi)$, in respect of the whole of the stars, is less than the mean found by Argelander from his second and third classes ; and if we leave out the two stars above mentioned near the pole, it is less even than that given by his first class, the values for his three classes being respectively 31° 31′·0, 32° 36′·6, 35° 41′·6.

It is difficult to form a satisfactory estimate of the probable accuracy of the result of this calculation, as compared with the results of Argelander and Otto Struve. The number of stars, though not large, might perhaps be regarded as sufficient to render the result worthy of confidence if the proper motions in right ascension and declination indicated by the comparison of the catalogues could be safely relied on ; but, unfortunately, in the present case, the probable errors of observation are hardly susceptible of exact appreciation, and the result is of course affected by the uncertainty of the data. With respect to the two recent catalogues, there is, indeed, no difficulty, inasmuch as the probable errors can be estimated with sufficient precision. Mr. Johnson considers the probable error in right ascension of a position given by the mean of five observations to be $0^s{\cdot}034\times\sec\delta$ in time (δ being the declination of

the star), or $0''{\cdot}51 \times \sec \delta$ in arc. Taking, as a mean value, $\delta = 45°$, this gives the probable error in right ascension $= 0''{\cdot}72$. He also states the probable error in declination, from five observations, to be $0''{\cdot}35$, exclusive of the error occasioned by uncertainty of refraction. Assuming the average error in declination from all causes to be the double of this, or $0''{\cdot}70$, we shall have the probable error in the place of the star, in the arc of a great circle, $= \sqrt{\{0''{\cdot}72^2 + 0''{\cdot}70^2\}} = 1''{\cdot}0$. In the case of HENDERSON's catalogue, the probable errors may be regarded as still smaller, owing to the superiority of the instruments of the Cape Observatory. But with respect to LACAILLE's observations, there is considerable uncertainty. His right ascensions were not determined, as in modern practice, by means of a transit instrument, but by the method of equal altitudes, with a 3-foot quadrant; and it is not certain whether the clock, on the accurate performance of which during the interval of the two observations of altitude the result mainly depends, was compensated for temperature. The declinations were observed with a 6-foot sector and a 6-foot sextant; and it is to be remembered that some of the most important elements of reduction—the aberration, nutation, refraction—were then imperfectly known. On the other hand, LACAILLE's well-known skill as an observer, the care he bestowed on the catalogue in the Fundamenta Astronomiæ, and the repeated examinations it has undergone by DELAMBRE and others, may be considered as rendering his positions trustworthy within limits which warrant their application to the purpose in hand. DELAMBRE, who had made extensive comparisons of LACAILLE's observations, estimated the probable error of one of his positions as double the probable error of one of BRADLEY's. But the probable error in declination of a star observed by BRADLEY is estimated by BESSEL at $0''{\cdot}7$; and the probable error in right ascension of an equatorial star, or, generally, the probable error of $\alpha \times \cos \delta$, is nearly the same as the probable error in declination; whence the probable error in the position of a star on the arc of a great circle may be taken at $\sqrt{2} \times 0{\cdot}7^2 = 0''{\cdot}94$, or less than one second. Assuming, then, the probable error of one of LACAILLE's positions to be $2''$, and that of one of JOHNSON's (as above shown) to be $1''$, the probable error of the difference of the catalogues becomes $\sqrt{4+1} = 2''{\cdot}236$; which divided by 80, the interval between the epochs, gives $0''{\cdot}028$ as the probable error of the annual proper motion deduced from the comparison of the two catalogues, so far as it depends on errors of observation. Hence it appears that a proper motion amounting to $0''{\cdot}1$ annually (the smallest which has been admitted in the present inquiry) considerably exceeds the probable errors of the catalogues, and consequently that the proper motions which have been under consideration not only have a real existence, but are determined with sufficient precision to give a result worthy of considerable confidence.

On the whole it may be said, that although the present result, if it stood by itself, would scarcely be considered as of sufficient weight to establish the fact and direction of the solar motion in space, yet coinciding as it does with those of ARGELANDER

and Otto Struve, it considerably increases the probability of the conclusions obtained by those astronomers. It is now shown that stars situated in every region of the heavens agree in their indication of a general motion directed towards one particular quarter; and as this agreement, not only of the results of different investigations, but of the great majority of the proper motions which have been ascertained and examined, cannot, on any reasonable supposition, be regarded as fortuitous, the inference is inevitable that they are, in part at least, systematic, and modified by the action of a general cause.

The proper motions which have been examined in this paper are not sufficiently numerous to warrant any speculation with respect to the nature of the path which the sun describes in space. Analogy leads us to infer that the sun must describe a curvilinear orbit, and if we suppose the orbit to be nearly circular, then the centre of motion will be situated in the plane passing through the sun perpendicular to the direction of his motion, and consequently in or near the great circle which has the point Q for one of its poles. The constellations through which this great circle passes are Piscis Australis, Pegasus, Andromeda, Perseus, &c. Argelander, from various considerations, thought it probable that the sun's orbit is nearly in the plane of the Milky Way, and therefore that the central body must be sought for in this plane also. Now the two points of the sphere in which the great circle which is 90° from Q (as above determined) intersects the plane of the Milky Way, are situated, the one in Perseus, $R = 49°$, Dec. $= +54\frac{1}{2}°$, and the other and diametrically opposite one between Lupus and the Southern Triangle. Near one of these two points, therefore, the central point of the sun's orbit must be situated, if both suppositions are correct; and Argelander considers it most probable that the central point or body is in Perseus. Mädler, in a recent remarkable speculation, comes to the conclusion that the central sun is most probably situated in the Pleiades, and nearly in the direction of the star Alcyone (η Tauri) of that group. Perhaps the research is at present premature; but it seems not unreasonable to expect that a comparison of catalogues at the end of another half century will give the means of answering many interesting questions connected with the proper motions of the stars for the determination of which the data are still insufficient. It may then be possible to determine, for example, whether the apparent proper motions are uniform, or variable as has been supposed by Pond and Bessel; whether the direction of the sun's proper motion is gradually changing, or the apex maintains a fixed position in the heavens; whether the stars, which appear so irregularly grouped, form different independent systems, each having its own centre of attraction, or all obey the influence of one controlling force which pervades the visible universe. The solution of all these questions will, no doubt, be ultimately arrived at, but much yet remains to be done by the practical astronomer. Our knowledge of the proper motions of the southern stars is still very defective; and unless some other means are adopted than those which have yet been had recourse to, namely, the comparison of absolute places at

distant epochs, a long time must elapse before the deficiency is supplied, and we may still say, in the words of HALLEY, that centuries may be required to discover the laws of the proper motions of the stars.

Method of Calculation.

The direction of the apparent motion of a star is conveniently defined by the angle which it makes with the circle of declination. Let S be the place of the star found by reducing the place given in the first catalogue to the epoch of the second, S′ its place given in the second catalogue, and P the north pole of the equator. Connecting these points by arcs of great circles, the arc SS′ represents the proper motion of the star in the interval between the epochs, and the angle PSS′ is the angle which has been denoted by ψ. This angle, which gives the direction of the star's motion, is reckoned from left to right all round the circle, from $\psi=0$ to $\psi=360^\circ$, and is computed from the variations of right ascension and declination indicated by the comparison of the catalogues as follows :—

Let α and δ denote respectively the right ascension and declination of the star at the mean epoch (1790), and $\Delta\alpha$, $\Delta\delta$ be the annual variations of those quantities arising from proper motion ($\Delta\alpha$ being in seconds of arc), and Δs the annual variation of the star's place in the arc of a great circle, we have then

$$\left.\begin{aligned} \Delta s \sin\psi &= \cos\delta\Delta\alpha \\ \Delta s \cos\psi &= \Delta\delta \\ \tan\psi &= \frac{\cos\delta\Delta\alpha}{\Delta\delta} \end{aligned}\right\} \quad \ldots\ldots\ldots\ldots \quad (1.)$$

The values of ψ and Δs calculated from these formulæ are given, for all the stars under consideration, in the appended table.

To determine the direction of the parallactic motion, let Q be the point towards which the sun's motion is assumed to be directed, T the point diametrically opposite, S the place of a star, P, P′ the north and south poles of the equator respectively, and let PSP′, PQP′ and QST be great circles of the sphere. In consequence of the real motion of the sun towards Q, the star, as seen from the earth, will appear to move towards T, in the great circle QST, the position of which will be given in terms of the angle PST which it makes with the declination circle PSP′. Let the angle PST be denoted by ψ', and let A and D denote respectively the assumed right ascension and declination of Q.

The angle ψ' may be computed immediately from the formula

$$\cot\psi' = -\cos SP'\cot QP'S + \frac{\sin SP'\cot QP'}{\sin QPS},$$

in which all the quantities are known, since SP′ is given in terms of δ, the star's declination, QP′ in terms of D, and the angle QPS represents the difference between α

and A, the right ascension of the star and of the point Q. It is more convenient, however, to compute the side QS (which is required in the subsequent calculations) in the first place, and to make use of it in computing ψ, because the same logarithms which are required in proceeding after this manner serve also for computing the coefficients of the equations of condition.

The ordinary trigonometrical formulæ give

$$\cos QS = \cos QP' \cos SP' + \sin QP' \sin SP' \cos QP'S.$$

Now since Q is assumed to be on the north side of the equator, and all the stars included in the present investigation are on the south side, we have

$\cos QP' = \cos(90° + D) = -\sin D$; $\sin QP' = \sin(90° + D) = +\cos D$.
$\cos SP' = \cos(90° - \delta) = +\sin\delta$; $\sin SP' = \sin(90° - \delta) = +\cos\delta$; $QP'S = \alpha - A$.

Denoting QS by χ, and substituting these values in the above formula, we get

$$\cos\chi = -\sin D \sin\delta + \cos D \cos\delta \cos(\alpha - A). \quad . \quad . \quad . \quad . \quad . \quad . \quad (2.)$$

Having found χ, or QS, the angle ψ is computed from the formula

$$\sin\psi = \frac{\sin QP' \sin QP'S}{\sin QS} = \frac{\cos D \sin(\alpha - A)}{\sin\chi}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3.)$$

This sine belongs to two angles. In general there will be no difficulty with respect to the quadrant to which it belongs; but in a case of ambiguity, which may occur when ψ is near 90°, recourse may be had to the formula for cot ψ' given above, which, on substituting for QP', SP', and QSP' their expressions in terms of D, δ, and $(\alpha - A)$, becomes

$$-\cot\psi' = +\sin\delta\cot(\alpha - A) + \frac{\tan D \cos\delta}{\sin(\alpha - A)} \quad . \quad . \quad . \quad . \quad . \quad . \quad (4.)$$

In computing from the above formulæ attention must be paid to the changes of sign of $\cos(\alpha - A)$ and $\sin(\alpha - A)$. The most convenient mode of proceeding, perhaps, is to take the stars in the order of right ascension, beginning at the declination circle, passing through Q, and adding 360° to all the values of α which are less than the assumed value of A, that is, to the right ascensions of all the stars excepting those which lie between the declination circle which passes through Q and that which passes through the first point of Aries. The values of $(\alpha - A)$ will thus be expressed in a series proceeding from 0° to 360°, and the sign to be prefixed to the cosine or sine becomes known from the value of the angle.

The equations of condition are formed as follows:—

Differentiating equation (4.) on the supposition that A and D are the variable quantities, we get

$$\frac{d\psi}{\sin^2\psi} = \frac{1}{\sin(\alpha - A)}\left\{\sin\delta + \tan D \cos\delta \cos(\alpha - A)\right\} dA + \frac{\cos\delta}{\sin(\alpha - A)\cos^2 D}\, dD;$$

now $\dfrac{\sin\psi}{\sin(\alpha - A)} = \dfrac{\cos D}{\sin\chi}$, therefore

$$d\psi = \frac{\cos D}{\sin^2\chi}\left\{\cos D \sin\delta + \sin D \cos\delta \cos(\alpha - A)\right\} dA + \frac{\cos\delta \sin(\alpha - A)}{\sin^2\chi}\, dD. \quad . \quad . \quad (5.)$$

If we now substitute for $d\psi$ the difference between the angles ψ and ψ', or the value of $\psi-\psi'$ as found by equations (1.) and (3.), we shall have an equation in which dA and dD are the only unknown quantities. Every star furnishes a similar equation; and the values of dA and dD deduced from the whole of the equations by the method of least squares give the corrections to be applied to A and D, the assumed right ascension and declination of Q. Before this method can be applied, however, it is necessary to consider how the observations are affected by the situation and other circumstances of the individual stars, in order that all the equations may be reduced to the same degree of precision.

In the present inquiry it is assumed that the positions given in the catalogues, and the reductions from the first epoch to the second, are equally precise for all the stars; and in respect of the true proper motion, it has already been stated that we are not possessed of data to enable us to make any distinction between one star and another, and must therefore assume that, in this respect, all the equations have the same weight. Confining our consideration, therefore, to that part of the apparent motion which is caused by the displacement of the sun, the relative weights of the equations are determined as follows:—

Let the parallactic motions in right ascension and declination, in the unit of time (here assumed to be one year), be denoted by $\Delta\alpha$ and $\Delta\delta$ respectively, and the corresponding motion in the arc of a great circle by Δs, and we have the equations

$$\Delta s \sin\psi' = \cos\delta\Delta\alpha\,;\quad \Delta s\cos\psi' = \Delta\delta,$$

by differentiating which we get

$$d\Delta s\sin\psi' + \Delta s\cos\psi' d\psi' = d(\cos\delta\Delta\alpha),$$
$$d\Delta s\cos\psi' - \Delta s\sin\psi' d\psi' = d\Delta\delta,$$

whence
$$\Delta s d\psi' = \cos\psi' d(\cos\delta\Delta\alpha) - \sin\psi' d\Delta\delta.$$

Denoting in general the probable error of any quantity x by $\varepsilon(x)$, and its square by $\varepsilon^2(x)$, and observing that if $x=y\pm z$ the theory of probable errors gives

$$\varepsilon(x) = \surd\{\varepsilon^2(y)+\varepsilon^2(z)\},$$

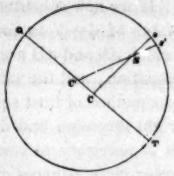
we have in respect of the above equation

$$\Delta s\,\varepsilon(\psi') = \surd\{\cos^2\psi'\varepsilon^2(\cos\delta\Delta\alpha) + \sin^2\psi'\varepsilon^2(\Delta\delta)\}.$$

Now the probable errors of $\Delta\alpha$ and $\Delta\delta$ depend manifestly on the precision and number of the observations from which the places of the stars have been determined; and as minute accuracy is not attainable in the present case, it may be assumed that the places of all the stars in each catalogue have been determined with equal precision. It may also be assumed that in respect of an equatorial star the probable error in right ascension is equal to the probable error in declination, and, generally, that $\varepsilon(\cos\delta\Delta\alpha)=\varepsilon(\Delta\delta)$. Denoting therefore the constant error by e, these two assumptions give $\varepsilon(\cos\delta\Delta\alpha)=\varepsilon(\Delta\delta)=e$, and the above equation becomes

$$\Delta s\,\varepsilon(\psi') = e\,;$$

whence it appears that the probable error of ψ' is inversely proportional to Δs.



To determine this quantity, let C be the place of the sun at the beginning, and C′ its place at the end of the time t, S the place of the star, and let straight lines joining CS and C′S meet the great circle whose plane contains the points C, C′, S, in s and s'. This plane also contains the points Q and T, which, therefore, are in the circumference of the same great circle. Now the angle CSC′, being the difference between QCS and QC′S, is the parallax, or angular variation of the apparent place of the star, which in consequence of the motion of the sun from C to C′ will appear to have moved from s to s', and is therefore (making $t=$ one year) the angle denoted by Δs. Hence the triangle CC′S gives

$$\sin \Delta s = \frac{\mathrm{CC'} \times \sin \mathrm{QC'S}}{\mathrm{CS}}.$$

Let CC′, which is constant, be denoted by R, and CS, the distance of the star, by r; then, observing that QC′S the angular distance of the star's place from Q is the angle which has been denoted by χ, and that $\sin \Delta s = \Delta s \sin 1''$, the above equation becomes

$$\Delta s = \frac{\mathrm{R} \sin \chi}{r \sin 1''}.$$

Substituting this in the equation $\Delta s\, s(\psi) = e$, we have

$$\frac{\mathrm{R} \sin \chi}{r \sin 1''} s(\psi) = e; \text{ or } \frac{\sin \chi}{r} s(\psi) = \text{a constant};$$

whence it follows that in order to reduce all the equations of condition to the same degree of precision, it is necessary to multiply each by $\sin \chi$, and to divide by a number proportional to r. The relative distances of the stars are however unknown, and in the present inquiry it has been assumed that they are all at the same mean distance, that is, r is assumed to be constant; and accordingly $\sin \chi$ becomes the measure of the precision of the equation.

Multiplying equation (5.) by $\sin \chi$, the formula for the equations of condition becomes

$$\sin \chi d\psi' = \frac{\cos \mathrm{D}}{\sin \chi} \left\{ \cos \mathrm{D} \sin \delta + \sin \mathrm{D} \cos \delta \cos (\alpha - \mathrm{A}) \right\} d\mathrm{A} + \frac{\cos \delta \sin (\alpha - \mathrm{A})}{\sin \chi} d\mathrm{D}; \quad . \quad . \quad (6.)$$

or, since

$$\frac{\cos \mathrm{D}}{\sin \chi} = \frac{\sin \psi}{\sin (\alpha - \mathrm{A})},$$

$$\sin \chi d\psi' = \frac{\sin \psi}{\sin (\alpha - \mathrm{A})} \left\{ \cos \mathrm{D} \sin \delta + \sin \mathrm{D} \cos \delta \cos (\alpha - \mathrm{A}) \right\} d\mathrm{A} + \frac{\cos \delta \sin \psi}{\cos \mathrm{D}} d\mathrm{D}. \quad . \quad . \quad (7.)$$

The double equation affords some advantage in checking the calculations; and it will be observed that the logarithms of all the sines and cosines required for computing the coefficients of dA and dD have already been used for computing χ and ψ'. It may be proper to state, that although in the appended table the logarithms of the sines and cosines of the different angles, and the coefficients of dA and dD in the

equations of conditions, are given to four decimal places, all the calculations were made from the tables of five-figure logarithms.

The weights and probable errors of the values of $d\mathrm{A}$ and $d\mathrm{D}$ deduced from the equations of condition by the method of least squares, and the probable error of Ψ, or probable value of $(\psi-\psi')\sin\chi$, are computed according to the following formulæ:

Putting the above equation (7.) under the form

$$0=ad\mathrm{A}+bd\mathrm{D}-n,$$

and assuming, according to the usual notation, (aa) to denote the sum of the squares of the coefficient a, (bb) the sum of the squares of b, (nn) the sum of the squares of n, (ab) the sum of the products ab, and so on, and denoting also the *weights* of $d\mathrm{A}$ and $d\mathrm{D}$ respectively by $w(d\mathrm{A})$, $w(d\mathrm{D})$, the formulæ of the method of least squares give

$$d\mathrm{A}=-\frac{(bb)(an)-(ab)(bn)}{(aa)(bb)-(ab)(ab)};\quad d\mathrm{D}=-\frac{(aa)(bn)-(ab)(an)}{(aa)(bb)-(ab)(ab)}$$

$$w(d\mathrm{A})=\frac{(aa)(bb)-(ab)(ab)}{(bb)};\quad w(d\mathrm{D})=\frac{(aa)(bb)-(ab)(ab)}{(aa)}.$$

Let h denote the number of equations, $\varepsilon(\Psi)$ the probable error of Ψ, $\varepsilon(d\mathrm{A})$ and $\varepsilon(d\mathrm{D})$ the probable errors of $d\mathrm{A}$ and $d\mathrm{D}$, and assume

$$(nn.2)=(nn)-\frac{(an)^2}{(aa)}-\frac{(bn.1)^2}{(bb.1)},$$

where

$$(bn.1)=(bn)-\frac{(ab)}{(aa)}(an);\quad (bb.1)=(bb)-\frac{(ab)}{(aa)}(ab),$$

then the theory gives

$$\varepsilon(\Psi)=\cdot67449\sqrt{\frac{(nn.2)}{h-2}};\qquad \varepsilon(d\mathrm{A})=\frac{\varepsilon(\Psi)}{\sqrt{w(d\mathrm{A})}};\qquad \varepsilon(d\mathrm{D})=\frac{\varepsilon(\Psi)}{\sqrt{w(d\mathrm{D})}}.$$

It may be remarked that in these formulæ $(nn.2)$ denotes the sum of the squares of the remaining errors when the values of $d\mathrm{A}$ and $d\mathrm{D}$, found as above, are substituted in the equations of condition.

In the following table (which has been frequently referred to) the names of the stars are given, with their mean places for 1790, and the principal details of the calculation. The columns headed "Lacaille — Johnson," "Lacaille — Henderson," "Bradley — Henderson," contain the proper motions as given by Mr. Johnson and Prof. Henderson in their respective catalogues for the whole interval between the compared catalogues, those in A being in seconds of time. In the column of difference in A the positive sign shows that the right ascension is greater in the ancient catalogue than in the modern, and has consequently been diminished, and the negative that it has been increased, through the effect of the proper motion. In the column of difference in declination, the positive sign indicates a proper motion towards the south, and the negative a proper motion towards the north; the stars being all in the southern hemisphere, and their declinations consequently supposed to be affected with the negative sign.

No.	Star's name.	Magnitude.	α (1790).	δ (1790).	Lacaille — Johnson.		Lacaille — Henderson.	
					Æ.	Dec.	Æ.	Dec.
			° ′	° ′	s	″	s	″
1	η Pavonis	4·5	261 17·6	64 35·5	+ 0·24	+14·0	.....	.....
2	ι¹ Scorpii	3	263 13·8	40 1·4	+ 0·24	+ 6·6	+ 0·57	+ 6·1
3	β Telescopii	4	270 51·3	36 48·3	+ 0·86	+14·6	.....	.....
4	ε Sagittarii	3	272 33·6	34 27·8	.....	.....	+ 0·79	+15·4
5	ζ Pavonis	4	274 36·5	71 34·4	+ 0·28	+14·4	.....	.....
6	β¹ Sagittarii	3·4	286 52·6	44 49·9	− 0·36	+ 9·6	.....	.....
7	α Sagittarii	4·5	287 19·6	40 59·5	− 0·64	+12·4	.....	.....
8	ε Pavonis	4	293 59·8	73 26·1	− 1·35	+ 8·3	.....	.....
9	δ Pavonis	4	296 59·3	66 41·5	−16·28	+92·4	.....	.....
10	α Pavonis	2	302 13·8	57 23·4	− 0·79	+ 4·7	.....	.....
11	α Indi	3	305 40·8	48 0·4	− 0·92	− 3·8	.....	.....
12	γ Pavonis	3	317 12·6	66 17·9	− 1·21	−60·6	− 1·08	−60·7
13	γ Gruis	3	325 17·4	38 20·6	− 0·81	+ 7·1	.....	.....
14	α Gruis	2	328 43·7	47 58·2	− 1·26	+17·2	− 1·07	+15·3
15	β Octantis	5	335 50·2	82 28·3	+ 5·85	+ 0·7	+ 7·23	+ 0·6
16	β Gruis	3	337 30·6	47 58·5	− 1·65	+ 4·1	− 1·54	+ 4·6
17	α Piscis Aust.	1	341 30·2	30 43·8	.....	.....	− 1·93	+16·6
18	γ¹ Octantis	5	354 48·1	83 11·1	+10·51	+ 0·3	+11·62	+ 0·83
19	Toucan	5	2 15·4	66 6·6	−20·95	−95·9	.....	.....
20	β Hydri	3	3 36·9	78 26·3	−57·38	−23·1	−59·41	−24·3
21	α Phœnicis	2	3 58·0	43 26·9	− 1·33	+36·4	− 1·15	+36·8
22	η Phœnicis	5	8 28·0	58 36·7	+ 1·10	+42·3	.....	.....
23	γ Phœnicis	3·4	19 48·4	44 23·9	+ 0·09	+20·9	+ 0·33	+20·6
24	δ Phœnicis	4	20 37·3	50 10·1	− 1·23	− 9·6	.....	.....
25	χ Eridani	4	26 56·8	52 39·6	− 4·84	−25·2	.....	.....
26	α Hydri	3·4	28 2·3	62 35·8	− 3·00	− 1·6	− 2·75	− 1·4
27	β Reticuli	4	55 24·5	65 28·3	− 2·43	−12·0	.....	.....
28	52 Eridani	3	66 51·0	31 0·2	.....	.....	+ 0·69	+ 5·1
29	β Columbæ	3	85 53·1	35 51·5	+ 0·04	−32·7	+ 0·33	−31·0
30	α Equ. Pict.	4	101 30·5	61 43·1	+ 1·65	−24·4	.....	.....
31	σ Argûs	4	110 38·7	42 53·1	+ 0·77	−10·6	.....	.....
32	γ² Argûs	2	120 46·0	46 43·5	+ 0·63	+ 6·0	+ 0·86	+ 5·0
33	ε Argûs	2	124 32·9	58 50·4	+ 0·03	− 7·3	+ 0·31	− 8·4
34	δ Argûs	3	129 43·7	53 56·7	+ 0·21	+12·6	+ 0·39	+11·9
35	α Pisc. Vol.	4·5	134 46·5	65 33·7	− 0·65	+10·4	.....	.....
36	G in C Argûs	5	136 7·4	71 45·1	+ 1·76	+43·5	.....	.....
37	β Argûs	1·2	137 42·5	68 51·3	+ 2·61	− 7·0	+ 2·53	− 8·7
38	δ² Chamæl.	5	160 54·8	79 26·0	+ 3·77	− 0·8	.....	.....
39	ε Chamæl.	5	177 22·0	77 3·1	+ 3·24	+ 5·1	.....	.....
40	ζ Centauri	3	179 23·3	49 33·1	+ 0·25	+ 7·9	+ 0·60	+ 7·3
41	δ Crucis	3	181 1·6	57 34·9	+ 0·60	− 2·0	+ 0·99	− 3·5
42	β Chamæl.	5	181 36·4	78 8·7	+ 3·24	+ 3·9	+ 3·79	+ 1·5
43	ε Crucis	4	182 32·2	59 14·4	+ 2·01	−13·7	.....	.....
44	α¹ Crucis	1	183 46·1	61 56·0	+ 1·64	− 5·9	+ 1·68	− 5·0
45	γ Crucis	2·3	184 54·5	55 56·2	− 0·56	+15·3	− 0·19	+15·7
46	γ Muscæ	4	185 2·6	70 58·3	+ 1·36	+ 5·3	.....	.....
47	γ Centauri	3	187 30·3	47 48·2	+ 1·56	+ 6·6	+ 1·96	+ 5·1
48	ι Centauri	3	197 12·8	35 36·0	+ 1·85	+ 8·4	+ 2·33	+ 9·4
49	ε Centauri	3	201 40·6	52 23·4	+ 0·58	+ 8·6	+ 1·03	+ 7·6
50	β Centauri	4·5	204 56·5	31 56·7	+ 0·83	+ 2·1	.....	.....
51	β Centauri	1	207 17·7	59 21·0	+ 0·51	+ 5·7	+ 0·95	+ 5·5
52	β Centauri	3	208 35·9	35 19·7	+ 3·42	+45·7	+ 3·87	+45·1
53	δ Octantis	5	208 50·3	82 40·9	+ 8·00	+ 2·5	+ 9·12	+ 1·2
54	ι Lupi	4·5	211 30·7	45 4·7	− 0·96	+ 5·6	.....	.....
55	α¹ Centauri	4	216 21·8	59 57·6	+37·21	−65·3	+38·44	−68·5
56	α Lupi	3	217 0·9	46 28·4	+ 0·16	+ 8·2	+ 0·63	+ 6·5
57	β Lupi	3	221 12·9	42 16·4	− 0·04	+12·6	+ 0·42	+11·5
58	π Lupi	5	222 43·7	46 12·8	+ 0·84	+ 7·8	.....	.....
59	γ Triang. Aust.	3	224 23·9	67 53·0	+ 1·33	+ 2·8	+ 2·03	+ 3·0
60	β Triang. Aust.	3	234 12·2	62 45·6	+ 1·88	+33·4	+ 2·45	+32·8
61	ε Scorpii	3	249 8·9	33 53·6	.....	.....	+ 4·15	+23·8
62	η Scorpii	3·4	254 17·2	42 56·4	− 0·28	+25·1	− 0·04	+25·0
63	β Aræ	3	256 58·3	55 18·4	− 0·25	+ 9·4	.....	.....
64	δ Aræ	4	258 3·0	60 28·8	+ 0·44	+ 8·5	.....	.....
65	α Aræ	3	258 54·5	49 41·1	− 0·24	+ 9·0	.....	.....

No.	Log $\Delta\alpha$.	Log $\Delta\delta$.	Δs.	ψ.	ψ'.	$\psi - \psi'$.	Log sin χ.
			″	° ′	° ′	° ′	
1	8·6532	9·2430	0·176	186 17·8	178 42·2	+ 7 35·6	9·9967
2	8·8692	8·8921	0·096	216 0·0	176 56·5	+ 39 3·5	9·9796
3	9·2075	9·2613	0·224	215 16·7	170 4·0	+ 45 12·7	9·9732
4	9·1546	9·2684	0·220	212 23·5	168 23·1	+ 44 0·4	9·9673
5	8·7202	9·2553	0·181	185 16·0	167 6·2	+ 18 9·8	9·9858
6	8·8293n	9·0792	0·129	158 15·2	157 6·7	+ 1 8·5	9·9948
7	9·0792n	9·1903	0·180	149 42·0	156 28·0	− 6 46·0	9·9900
8	9·4033n	9·0160	0·127	145 7·0	150 0·0	− 4 53·0	9·9772
9	0·4847n	0·0626	1·671	133 43·2	148 24·3	− 14 41·1	9·9885
10	9·1706n	8·7690	0·099	126 21·1	144 59·3	− 18 38·2	9·9967
11	9·2368n	8·6767n	0·125	67 37·8	142 42·7	− 75 4·9	0·0000
12	9·3243n	9·8718n	0·749	6 30·1	131 37·5	−125 7·4	9·9782
13	9·1815n	8·9482	0·149	126 41·2	129 44·4	− 3 3·2	9·9992
14	9·3321n	9·3003	0·246	144 14·1	126 36·0	+ 17 38·1	9·9915
15	0·1167	7·9026	0·172	267 19·8	108 21·8	+158 58·0	9·9358
16	9·4681n	8·7271	0·204	105 10·3	120 52·2	− 15 41·9	9·9824
17	9·5426n	9·3010	0·360	123 42·4	122 6·6	+ 1 35·8	9·9936
18	0·3086	7·3757	0·242	268 33·3	89 20·3	+179 13·0	9·9244
19	0·5942n	0·0787n	1·992	53 0·0	93 32·0	− 40 32·0	9·9165
20	1·0313n	9·4635n	2·175	82 18·8	83 43·5	− 1 24·7	9·9159
21	9·3591n	9·6524	0·479	159 43·3	106 51·2	+ 52 52·1	9·9317
22	9·3144	9·7233	0·540	191 29·0	93 30·0	+ 97 59·0	9·9033
23	8·5827	9·4061	0·256	186 7·5	96 54·0	+ 89 13·5	9·8666
24	9·3629n	9·0792n	0·190	50 54·7	90 10·0	− 39 15·3	9·8552
25	9·9579n	9·4983n	0·634	60 13·1	83 16·0	− 23 2·9	9·8304
26	9·7241n	8·2655n	0·244	85 40·6	71 54·0	+ 13 46·6	9·8431
27	9·6586n	9·1761n	0·241	51 35·1	36 10·0	+ 15 25·1	9·7705
28	9·0959	8·7885	0·123	240 6·3	101 11·0	+138 55·3	9·2838
29	8·5260	9·5923n	0·392	356 1·3	301 52·9	+ 54 8·4	9·0240
30	9·4905	9·4843n	0·338	334 19·9	324 20·3	+ 9 59·6	9·7291
31	9·1595	9·1222n	0·170	321 23·8	282 53·0	+ 38 30·8	9·6474
32	9·1360	8·8301	0·116	234 12·0	279 47·5	− 45 35·5	9·7493
33	8·4891	8·9833n	0·098	350 35·0	291 31·4	+ 59 3·6	9·8053
34	8·7398	9·1774	0·154	192 7·7	280 44·5	− 88 36·8	9·8177
35	9·0859n	9·1139	0·139	158 48·0	287 31·2	−128 43·2	9·8601
36	9·5185	9·7354	0·554	190 45·6	291 25·9	−100 40·3	9·8775
37	9·6751	8·9830n	0·196	299 23·3	287 23·7	+ 11 59·6	9·8745
38	9·8493	8·0000n	0·130	274 24·7	272 1·0	+ 2 23·7	9·9211
39	9·7836	8·8045	0·150	244 54·3	254 40·7	− 9 46·4	9·9380
40	8·8901	8·9701	0·106	208 21·2	241 16·6	− 32 55·4	9·9769
41	9·1635	8·5261n	0·085	293 15·7	242 41·9	+ 50 33·8	9·9689
42	9·8104	8·5239	0·137	255 52·5	251 17·0	+ 4 35·5	9·9403
43	9·5762	9·2336n	0·258	311 37·2	242 8·0	+ 69 29·2	9·9687
44	9·4851	8·8260n	0·159	294 59·0	242 7·5	+ 52 51·5	9·9665
45	8·8431n	9·2792	0·194	168 24·4	239 18·1	− 70 53·7	9·9763
46	9·4065	8·8212	0·106	231 27·0	244 40·0	− 13 13·0	9·9544
47	9·5097	8·8572	0·229	251 40·0	235 33·0	+ 16 7·0	9·9887
48	9·5847	9·0379	0·331	250 44·0	228 27·8	+ 22 16·2	0·0000
49	9·1687	8·9980	0·134	222 7·0	226 26·2	− 4 19·2	9·9948
50	9·1921	8·4191	0·135	258 45·5	224 2·5	+ 34 43·0	9·9964
51	9·1258	8·8373	0·097	224 44·5	223 3·5	+ 1 41·0	9·9911
52	9·8263	9·7461	0·781	224 27·5	221 26·7	+ 3 0·8	9·9968
53	0·1970	8·3591	0·202	263 29·7	227 42·5	+ 35 47·2	9·9471
54	9·2553n	8·8451	0·145	118 50·6	219 0·5	−100 9·9	9·9999
55	0·8427	9·9142n	3·581	283 15·1	215 56·8	+ 67 18·3	9·9945
56	8·8569	8·9562	0·103	208 43·3	214 57·5	− 6 14·2	9·9997
57	8·5340	9·1703	0·150	189 42·1	212 0·2	− 22 18·1	9·9965
58	9·1973	8·9890	0·146	228 11·1	210 38·5	+ 17 32·6	9·9987
59	9·4887	8·5511	0·121	252 57·2	210 10·2	+ 42 47·0	9·9875
60	9·5995	9·6089	0·445	204 6·7	201 32·2	+ 2 34·5	9·9963
61	9·8751	9·4575	0·686	245 16·2	189 42·8	+ 55 33·4	9·9645
62	8·4752n	9·4878	0·308	175 55·9	184 46·4	− 8 50·5	9·9861
63	8·6709n	9·0700	0·121	167 12·4	182 21·7	− 15 9·3	9·9997
64	8·9165	9·0263	0·114	200 56·2	181 27·2	+ 19 29·0	9·9994
65	8·6532n	9·0512	0·116	165 29·4	180 44·0	− 15 14·6	9·9960

No.	Star's name.	α (1790).	δ (1790).	Bradley — Henderson.	
				Æ.	Dec.
				s	″
66	β Ceti	8 15·6	19 8·5	−1·10	− 1·5
67	θ¹ Ceti	18 23·0	9 16·4	+0·41	+16·9
68	ζ Ceti	25 16·5	11 23·7	−0·05	+10·6
69	η Eridani	41 32·7	9 44·6	−0·47	+17·6
70	γ¹ Eridani	57 3·6	14 7·0	−0·37	+ 8·1
71	β Eridani	74 23·0	5 22·3	+0·46	+ 5·9
72	α Canis Maj.	98 58·3	16 26·6	+2·67	+97·2
73	15 Argûs	119 37·4	23 37·2	+0·41	− 5·9
74	β Hydræ et Crat.	167 13·1	13 38·7	+0·53	−13·1
75	γ Corvi	181 15·8	16 22·6	+0·90	− 1·7
76	δ Corvi	184 45·2	15 20·7	+0·43	+12·1
77	β Corvi	185 51·0	22 19·0	+0·55	+ 5·7
78	α¹ Libræ	219 51·5	15 6·3	+0·51	+ 5·8
79	δ Ophiuchi	240 50·4	3 8·5	+0·32	+ 9·5
80	η Ophiuchi	254 36·6	15 27·0	−0·13	− 8·3
81	σ Sagittarii	281 31·3	26 32·5	−0·15	+ 7·6

No.	Log Δα.	Log Δδ.	Δs.	ψ.	ψ′.	ψ−ψ′.	Log sin χ.
			″	° ′	° ′	° ′	
66	9·3254*n*	8·2840*n*	0·201	84 30·2	117 41·5	− 33 11·3	9·9559
67	8·8968	9·3358	0·230	199 45·3	122 8·1	+ 77 37·2	9·9417
68	7·9830*n*	9·1332	0·136	176 2·0	122 2·9	+ 53 59·1	9·9085
69	8·9561*n*	9·3534	0·243	158 27·4	128 39·0	+ 29 48·4	9·8249
70	8·8522*n*	9·0164	0·125	146 23·8	135 28·6	+ 10 55·2	9·6669
71	8·9468	8·8788	0·116	229 20·6	170 9·2	+ 59 11·4	9·6656
72	9·7105	0·0956	1·340	201 33·8	223 45·1	− 22 11·3	9·6033
73	8·8968	8·8788*n*	0·105	316 19·0	246 42·5	+ 69 36·5	9·7697
74	9·0083	9·2252*n*	0·195	329 28·2	238 39·4	+ 90 48·8	9·9941
75	9·2382	8·3384*n*	0·167	277 28·6	235 45·0	+ 41 43·6	0·0000
76	8·9175	9·1907	0·174	207 12·4	234 45·2	− 27 32·8	9·9990
77	9·0244	8·8638	0·122	233 14·8	234 8·8	− 0 54·0	0·0000
78	8·9916	8·8713	0·120	231 15·5	218 13·1	+ 13 2·4	9·9419
79	8·7892	9·0856	0·136	206 46·3	205 15·2	+ 1 31·1	9·8071
80	8·3979*n*	9·0270*n*	0·109	12 45·6	185 50·5	−173 4·9	9·8724
81	8·4601*n*	8·9887	0·101	165 10·0	160 15·9	+ 4 54·1	9·9470

Equations of Condition.

From comparison with Lacaille's Catalogue.

1	0 = + ·8434dA	+ ·0115dD	− 7·54	34	0 = + ·6140dA	− ·6856dD	+ 58·24
2	0 = + ·8425	+ ·0484	− 37·27	35	0 = + ·7455	− ·4678	+ 93·27
3	0 = + ·8321	+ ·1638	− 42·50	36	0 = + ·7921	− ·3456	+ 75·94
4	0 = + ·8269	+ ·1968	− 40·81	37	0 = + ·7700	− ·4081	− 8·98
5	0 = + ·8405	+ ·0837	− 17·58	38	0 = + ·8233	− ·2173	− 2·00
6	0 = + ·7971	+ ·3270	− 1·13	39	0 = + ·8153	− ·2562	+ 8·47
7	0 = + ·7878	+ ·3573	+ 6·61	40	0 = + ·6227	− ·6745	+ 31·22
8	0 = + ·8313	+ ·1690	+ 4·63	41	0 = + ·6960	− ·5648	− 47·07
9	0 = + ·8176	+ ·2458	+ 14·30	42	0 = + ·8207	− ·2307	− 4·00
10	0 = + ·7868	+ ·3666	+ 18·50	43	0 = + ·7102	− ·5361	− 64·66
11	0 = + ·7397	+ ·4805	+ 75·08	44	0 = + ·7338	− ·4391	− 48·94
12	0 = + ·7880	+ ·3563	+ 119·00	45	0 = + ·6924	− ·5710	+ 67·12
13	0 = + ·5896	+ ·7151	+ 3·05	46	0 = + ·7903	− ·3494	+ 11·90
14	0 = + ·6500	+ ·6373	− 17·29	47	0 = + ·6361	− ·6567	− 15·70
15	0 = + ·8342	+ ·1474	− 137·12	48	0 = + ·5839	− ·7216	− 22·27
16	0 = + ·6174	+ ·6813	+ 15·07	49	0 = + ·7182	− ·5243	+ 4·27
17	0 = + ·4257	+ ·8632	− 1·57	50	0 = + ·6028	− ·6994	− 34·43
18	0 = + ·8351	+ ·1406	− 150·79	51	0 = + ·7683	− ·4127	− 1·65
19	0 = + ·7403	+ ·4792	+ 33·44	52	0 = + ·6479	− ·6403	− 2·99
20	0 = + ·8196	+ ·2362	+ 1·16	53	0 = + ·8381	− ·1117	− 3·68
21	0 = + ·4782	+ ·8238	− 45·20	54	0 = + ·7169	− ·5270	+ 100·15
22	0 = + ·6642	+ ·6164	− 78·46	55	0 = + ·7906	− ·3484	− 66·46
23	0 = + ·4564	+ ·8410	− 65·63	56	0 = + ·7433	− ·4677	+ 6·23
24	0 = + ·5488	+ ·7594	+ 28·12	57	0 = + ·7467	− ·4649	+ 22·12
25	0 = + ·5904	+ ·7141	+ 15·60	58	0 = + ·7662	− ·4181	− 17·49
26	0 = + ·7210	+ ·5187	− 9·60	59	0 = + ·8226	− ·2843	− 41·57
27	0 = + ·8071	+ ·2905	− 9·09	60	0 = + ·8266	− ·1992	− 2·53
28	0 = + ·0630	− ·9810	+ 26·71	61	0 = + ·8317	− ·1661	− 51·19
29	0 = + ·4883	− ·8169	− 5·72	62	0 = + ·8412	− ·0722	+ 8·55
30	0 = + ·7970	− ·3275	− 5·36	63	0 = + ·8431	− ·0278	+ 15·14
31	0 = + ·4486	− ·8469	− 17·10	64	0 = + ·8434	− ·0148	− 19·46
32	0 = + ·5050	− ·8009	+ 25·60	65	0 = + ·8434	− ·0098	+ 15·10
33	0 = + ·6926	− ·5707	− 37·72				

From comparison with Bradley's Catalogue.

66	0 = + ·1080dA	+ ·9918dD	+ 29·98	74	0 = + ·1502dA	− ·9840dD	− 89·60
67	0 = + ·1138	− ·9908	+ 67·87	75	0 = + ·2872	− ·9404	− 41·72
68	0 = + ·1449	− ·9851	+ 43·73	76	0 = + ·3020	− ·9338	+ 27·48
69	0 = + ·3448	− ·9125	+ 19·92	77	0 = + ·3863	− ·8890	+ 0·90
70	0 = + ·4992	− ·8062	+ 5·07	78	4 = + ·5955	− ·7082	− 11·41
71	0 = + ·8264	− ·2019	+ 27·41	79	0 = + ·7280	− ·5050	− 0·97
72	0 = + ·5211	+ ·7864	− 8·90	80	0 = + ·8377	− ·1163	+ 129·57
73	0 = + ·0572	+ ·9978	+ 40·96	81	0 = + ·7875	+ ·3581	− 4·34

X. *On Photographic Self-registering Meteorological and Magnetical Instruments.*

By Francis Ronalds, *Esq., F.R.S.*

Received November 18, 1846,—Read January 21, 1847.

THE Council of the Royal Society having, in April last, granted to me, out of the Donation Fund, fifty pounds "for the purchase of magnetical and meteorological instruments" necessary to the prosecution of experiments in which I was then (and had been long previously) engaged, I feel very anxious to express, now, my deep sense of obligation for its munificence, and to endeavour to show that successful and satisfactory results, in great measure due to the timely assistance thus kindly afforded, have been attained.

It would be superfluous to speak of those proposals of other gentlemen, or of my own, for self-registering, photographically, the variations of the declination magnet and the thermometer which were made previously to *the use of good achromatic lenses, for projecting, upon photographic paper, a sharp image, magnified to any required degree, of that part of the instrument whose motions are to be registered.*

This is the principal feature of the system which I have hitherto applied to procure the self-registration of the atmospheric electrometer, the thermometer, the barometer, and the declination magnetometer, and which I propose to apply to every other meteorological and terrestro-magnetical instrument. Although it had long occupied my thoughts, had received some approbation from the Astronomer Royal in April 1845, and had been the object in view in some experiments made in conjunction with Mr. H. Collen of Somerset Street, on photographic paper in the beginning of July 1845, yet my other contrivances and occupations at the Kew Observatory (elsewhere detailed) prevented the completion of any apparatus for actual registration until the end of that month.

The photo-electrograph as then constructed, and since improved, may be thus described.

A rectangular box, about 16 inches long and 3 inches square, constitutes the part usually called the "body" of a kind of lucernal microscope. A voltaic electrometer (properly insulated, and in communication with an atmospheric conductor) is suspended within this microscope, through an aperture in the upper side, and near to the *object* end. That end itself is closed by a plane of glass, when daylight is used, and by condensing lenses, when a common Argand lamp is employed. In either case an abundance of light is thrown into the microscope. Between the electrometer and the other, or *eye-end* of the microscope, fine achromatic lenses are placed, which have the double effect of condensing the light upon a small screen, situated at that eye-end, and of

projecting a strong image of the electrometer, in deep *oscuro*, upon it. Through the screen a very narrow slit, of proper curvature, is cut (the chord of the arc being in a horizontal position), and it is fitted into the back of a case, about two and a half feet long, which case is fixed to the eye-end of the microscope, at right angles with its axis, and vertically. Within this case is suspended a frame, provided with grooves, into which two plates of pure thin glass can be dropped, and brought into close contact by means of six little bolts and nuts. This frame can be removed at pleasure from a line, by which it is suspended, and the line after passing through a small aperture (stopped with grease) cut through the upper end of the long case, is attached to a pulley (about 4 inches in diameter) fixed, with capacity of adjustment, on the hour-arbor of a good clock. Lastly, counterpoises, rollers, springs, and a straight ruler are employed, for ensuring accurate rectilineal sliding of the frame when the clock is set in motion.

A piece of properly prepared photographic paper is now placed between the two plates of glass in the moveable frame; the frame is removed (in a box made purposely, for excluding light) and is suspended in the long case; this is closed so as to prevent the possibility of extraneous light entering it; the clock is started; and the time of starting is noted.

All that part of the paper which is made to pass over the slit in the screen, by the motion of the clock, becomes now therefore successively exposed to a strong light; and is consequently brought into a state which fits it to receive a dark colour on being again washed with the usual solutions, *excepting* those small portions upon which dark images of the lower parts of the pendulums of the electrometer are projected through the slit; these small portions of course retain the light colour of the paper; and form the long curved lines or bands, whose distances from each other at any given part of the photograph (*i. e.*), at any given time, indicate the electric tension of the atmosphere at that time.

Sometimes, when daylight was used, various appearances of the sky were noted during the process, by which it would seem, that, in serene weather, when the sun's light and heat varied, and the paper became consequently more or less darkened, the electric tension, as shown in the photograph, varied also; increasing with the increment of light, &c. This fact has not perhaps been before observed: and some attentive observations on the subject, made with the aid of a *good* actinometer, &c., are desirable.

In order that the state of the electrometer itself may be known at any period of the process, a small microscope is fitted to an aperture in the door of the long case, opposite to the slit in the screen, and arrangements are made whereby the eye may be applied to it, and to view the images through the semitransparent photographic paper, without damage by the admission of extraneous light.

The adjustment of the lenses in the body of the lucernal microscope, for procuring the best possible *chemical* focus, can only be obtained by a short series of experiments; but having been once found, future adjustments are not necessary.

In pursuance of a hint or suggestion of the Astronomer Royal, a very useful addition to the electrometer has been made, by which *the kind* in addition to the *tension* of the electrical charge is registered. It depends upon the same principle as that of the "*Dry Pile* distinguishing apparatus," and will be clearly understood by reference to the diagram.

The above-described instrument, with its various improvements, made during the progress of the experiments, was used in connection with an atmospheric conductor situated much lower, and otherwise much less advantageously than our ordinary Kew-conductor*.

In order to adapt the greater part of the apparatus to the purposes of a *thermograph*, a thermometer of the horizontal kind, and having a flat bore, is employed. Its tube is introduced through a side of the microscope in lieu of the electrometer; a diaphragm is fixed upon it of narrower dimensions than the breadth of the mercury; and the slit in the screen at the eye-end of the microscope is (of course) straight and horizontal; the manipulation and all else remaining as before.

The photo-barometrograph requires a somewhat different arrangement of the same microscope, &c.

The long case in which the frame carrying the photographic paper slides, is now placed in a horizontal position. The clock with its pulley, &c. is fixed near to one end of that case: the usual line, attached to the pulley on the clock-arbor, enters the case, as before, and is hooked to the sliding frame, and provision is made of the same kind as before, &c. for ensuring its steady and regular motion.

The lower leg of a siphon barometer is introduced through the *now* lower side of the microscope (in lieu of the thermometric tube), and a very light blackened pith-ball rests upon the surface of the mercury. In order to procure a clean and correct boundary-line in the photograph, a kind of contracting and expanding diaphragm on the barometer tube became very useful in this case, for when daylight was used, very minute adjustments of the aperture were required. The slit in the screen is (of course) vertical. The curves seen in the photographs represent the actual variations nearly of this *siphon* barometer, but it is my intention to try to fill a barometer of the *cistern* kind, in the late Professor DANIELL's manner, with a pith-ball on the surface of the mercury still, and then to use a magnified range of the image.

The curve or right line which forms the right-hand boundary of the dark band, seen in the photographs of the thermometer and barometer, represents the variations of the mercury as accurately as the usual scale readings of ordinary instruments.

The applicability of this system of self-registration to a *magnetograph* was sufficiently obvious; but a much more solid kind and disposition of apparatus is necessary.

The two-feet magnet, now used for this purpose at the Kew Observatory, was kindly lent to me by the Astronomer Royal (in February 1846), and is suspended by

* The photographs executed between July 24, 1845, and August 29, 1846, by means of this and my various other instruments, were produced at the meeting of the Royal Society, at which this communication was read.

a silken skein of the same kind as that used for suspending the Greenwich declination magnet. The damper surrounding the magnet is of mahogany coated (by means of the electrotype process) with pure copper, and the mode of suspension is *essentially* similar to that of Coulomb. The length of the skein from the magnet to the point of suspension is nine feet, and the detorsion plate is supported by braced frame-work, fixed firmly by brass bolts upon the two pillars which formerly carried the Kew transit instrument. The interior and exterior cases containing the magnet are coated entirely with gold paper, as are those of the Greenwich magnets.

A light conical brass tube projecting about six inches beyond the north end of the magnet, is affixed to the lower side of the stirrup which carries the magnet. Into this end slides (for adjustments) a small cylindrical tube, and from the extremity of the latter descends, vertically, a very small blackened wire (called the index) which passing through slits (long enough for its free motion always) cut through the bottoms of the cases, takes the place of the above-described electrometer, &c. in a lucernal microscope below.

The lucernal microscope is in this instance much longer than before, in order that the motion of the index may be considerably magnified in its image, without sensible error of aberration, &c. The long case (now vertical) with its sliding frame, &c., have the usual form and dimensions; and the clock, with brass pendulum, weight, &c., is placed at a proper distance from the magnet.

I have taken great pains to prevent internal and external currents of air, but the magnet is far from being as steady as the Greenwich magnets, particularly during strong westerly and north-westerly winds. If it were as little subject to mechanical disturbances as the Greenwich magnets, I feel quite sure that the photographs would present as sharp outlines as those of the barometer, electrometer, &c.

Concerning the impressions which have been submitted to the Astronomer Royal's inspection, and compared with the readings of the Greenwich declinometer, I am permitted kindly by Mr. Airy to say, that the agreement of those results with such readings is highly satisfactory.

Postscript.—January 7, 1847.

Since writing the above I have felt strongly impressed with the great advantage which would result in these kind of registrations of the barometer, if that instrument could be rendered *accurately* self-corrective for temperature; and I have been much occupied with a project of this kind (promising very fairly). It consists in applying a solid metallic thermometer in such manner as to cause the whole barometer to descend exactly as much as increments of temperature cause the mercury to ascend.

Explanation of the Plates.

PLATE X.

Fig. 1. *Electrograph.*

A. The body of the lucernal microscope; its nearest side removed.

B. The voltaic electrometer; the pendulums b^1 suspended by hooks from an insulated piece b^2.

C. A board fitted to the end of A, and having a curved diaphragm (c^1) cut through it. The condensing lens is placed beyond this diaphragm (and not visible).

D. The Argand fountain lamp, or a camphine lamp.

E. The screen, with its slit (e^1).

F. The case which receives the sliding-frame. f^5. Springs pressing the glass of the frame (H. fig. 2) against E when the door is closed.

G. The tube containing the achromatic lenses.

Fig. 2.

H. The sliding frame suspended in F. h^1, h^1, &c. Bolts and nuts for pressing the photographic paper between the two plates of glass. h^2, h^2. Friction-rollers. f^3. Spring for pressing H against f^2, the ruler. h^3. Hooks.

I. The pulley on the hour-arbor of the clock. i^1. The line (of gut) suspending H; i^5. the counterpoise (to H) suspended by i^3; i^2, and i^4, terminations of i^1 and i^3.

K. The time-piece.

L. The transfer-box.

Fig. 3. *Thermometrograph.*

A. Section of the lucernal microscope.

B. Thermometer; b^1, its tube; b^2, its bulb. b^3. Diaphragm fixed upon b^1.

Fig. 4. *Barometrograph.*

A. Section of the microscope.

B. Siphon barometer. b^1. Black pith-ball resting on the mercury.

Fig. 5. *Contracting Diaphragm.*

M. Plate having a slit cut through its central part.

N. Small parallel ruler (of the common kind). n^1 and n^2. Screws which pass through the centres of its cross-bars (with friction) and screw firmly into M in the line of the centre of the slit.

Fig. 6. *Distinguishing Electrograph.*

A. Section of the microscope.

B. The voltaic electrometer. b^1. The pendulums suspended from the piece b^2. b^5. A little plate attached to the lower end of b^2 between the pendulums. b^6. A longer pendulum suspended by a hook from b^5, and

passing through a large aperture in the bottom of A. b^7. A pith-ball fixed on b^5. b^8, b^8. Two small Leyden jars, of *very* thin glass, fitted upon sliders, which can be made to approach or recede from each other by means of adjusting screws. b^9, b^9. Wires connected with their interior coatings, and capable of adjustment for height. b^{10}, b^{10}. Cross wires, each carrying at one end a brass ball, and at the other a small electroscope, of straws.

When this distinguishing electrometer is in use, the jars are charged artificially, one negatively, the other positively: consequently, if B receives a positive charge from the atmospheric conductor, b^7 is attracted toward the negative ball, and b^6 inclines in that direction, and *vice versâ*.

It is evident, therefore, that in the resulting photograph a line must appear between the two lines, produced by the images of the short straws b^1, b^1, but nearer to one than to the other. In order to ascertain with certainty, in low intensities, to which ball it inclines, a very small bar (e^3, fig. 7) is placed across the exact centre of the slit in the screen; the *shadow* of which bar (e^2) of course creates a central line in the photograph.

These jars retain a *sufficient low* charge for twenty-four hours even in a humid state of the air (for they are somewhat similarly circumstanced to an electrophorus, which will frequently retain a low charge for one or *two* months).

Fig. 7. The screen E (*vide* fig. 1) with its slit e^1, the visible parts of the images of the short straws b^1, b^1, that of the long straw b^6, and the bisecting little cross-bar e^2.

PLATE XI.

Fig. 8. *The Declination Magnetograph.*

AV. A box divided into two compartments.

W. An interior box.

A. The lucernal microscope.

B. The declination magnet. b^2. Its stirrup, from which it may be removed at pleasure, and the usual brass bar substituted after relaxing two milled-headed screws and turning downward the nearest sides of the stirrup. b^3. The pair of light tubes connected with the stirrup by means of a short tube, which permits a horizontal adjustment of b^3. The counterbalance on the nearer end of b^3 is also adjustible.

b^1. The index. b^4. The damper, the central parts of which, above and below, are expanded and form rings.

C. The condensing lens (or a glass plane), beyond which a brass lamp (*vide* fig. 1), or when daylight is used, a mirror is placed.

G. The tube containing the achromatic lenses.

O. A diaphragm.

E. The screen. e^1. The slit cut through it.

F. The case for reception of the sliding frame (*vide* H, fig. 2).

IK. The pulley, clock, &c. (*vide* fig. 2.)

PP. The two pillars of the late transit instrument at Kew.

QQQ. Parts of the frame-work and braces fixed upon PP.

XX. Bearers clamped firmly upon PP by means of cross-bars, x^1, x^1, and wedges, x^2, &c.

A plumb-line, not visible in the sketch, is suspended from the summit of the frame-work and descends to X for the purpose of ascertaining whether any appreciable movement of the point of suspension has occurred. I have not yet discovered any such movement.

Fig. 9.

R. One of the two bearers which unite the upper opposite ends of QQ (fig. 8).

S. Section of the detorsion-plate, &c. s^1. A rectangular plate which can be slid between grooved pieces fixed upon the bearers (R) and accurately adjusted by means of the screws s^2, s^2 passing through the pieces s^3, s^3. s^4. The graduated circular plate with its central neck descending through a socket in s^1*. On s^4 a pulley is supported, which is provided with milled-headed nuts on its prolonged axis, serving to secure it from revolution at pleasure. s^5. The silken skein attached to the pulley and descending through a small aperture in the neck of s^4.

T. The upper part of a narrow box inclosing the skein. The lower part is seen at T, fig. 8.

* I propose to avail myself of Sir John Herschel's obliging suggestion of turning, with *extreme* slowness and equability, this detorsion-plate on its centre by means of a tangent screw, moved by simple apparatus worked *below*, and whilst the effect on the brass bar may be watched by the operator, even when the boxes are closed.